ON THE DISCREPANCY BETWEEN GRATING AND INTERFERENCE MEASUREMENTS.

By Louis Bell.

For several years past it has been well understood that there is a wide discrepancy between the value for absolute wave-length on which Rowland's maps are based and that obtained by Michelson through the use of his beautiful interference methods. Moreover, the difference is of a size and character that, as I shall presently show, precludes its being properly chargeable to experimental errors of either method or to differences in the standards of length to which these measurements have been referred.

The first step toward an explanation is to determine the actual amount of the difference as it stands and the extent to which it may be influenced by known sources of error. This is not so easy as it would seem to be. Rowland's standard wave-lengths[1] are based upon an assumed weighted mean value for the wave-length of D_1, derived in the main from the writer's grating measurements.[2]. They were obtained by the coincidence method, applied with enormous skill and care. The precision of

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[1] A. and A., **12**, 321, 1893.   [2] Phil. Mag., (5) **25**, 255, 350, 1888.

measurement is slightly different for different lines, but in general the weight of the evidence indicates that the error in case of the main solar standards will rarely exceed 0.005 tenth-meter and ordinarily fall within 0.002 tenth-meter. Michelson's wave-lengths,[1] on the other hand, were obtained with vacuum tubes charged with *Cd* vapor as a source of light. These lines are very weak and dubious as reversed in the solar spectrum, and are not among Rowland's solar standards. They are, however, given as standards measured from the arc, and thus a connection is established between the absolute values of wave-length as obtained by the two methods in question.

Another path to the same result is that established by Perot and Fabry[2] in their estimation of certain lines of *Hg*, *Zn*, *Cu*, *Ag*, *Na*, and *Li* compared directly by the interference method with Michelson's red cadmium line. Unhappily the lines of the first named element are not definitely present in the solar spectrum, and those due to the other elements measured are, except in the case of *Na*, few and weak. The D lines, three lines of *Zn* at $\lambda 4680$, 4722, and 4810, and a faint *Cu* line at $\lambda 5105$ are available for comparison, and Rowland has the red *Li* line as a fairly good arc standard.

All the Perot and Fabry figures, like those of Michelson, are for 15° C. and 760 mm, so that before they can be compared with the Rowland tables the latter, standard at 20° C. and 760 mm, must be reduced for temperature difference.

For this purpose I have followed the values of Lorenz[3] for the refraction and dispersion of air. On this basis the wave-length of D_1 as used by Rowland, becomes, at 15° C. and 760 mm

$$D_1 = 5896.132.$$

Reducing in similar fashion other Rowland standards to 15° C., and deriving from them the value of D_1 by comparison with the wave-lengths established through the interference methods, one

[1] *Mém. du B. int. des Poids et Measures*, II, 1895.

[2] *C. R.*, **130**, 492, 1900.

[3] *Wied. Ann.*, **11**, 70, 1880.

can derive the following table of values for D_1 based on inter-
ference measurements:

Derivation of wave-length	Wave-length of D_1	Difference from mean	Remarks
M., absolute Cd_R, via R. Table of Standards	5895.967	+0.027	⎞ Comparison is with
M., absolute Cd_G, via R. Table of Standards......	5895.950	+0.010	⎟ arc lines in R.
M., absolute Cd_B, via R. Table of Standards......	5895.926	—0.014	⎠ Tables.
P. & F., Zn 4680, M. Cd_R, via R. Table of Standards	5895.923	—0.017	⎞ Comparison is with
P. & F., Zn 4722, M. Cd_R, via R. Table of Standards	5895.925	—0.015	⎟ R. solar standard, P.
P. & F., Zn 4810, M. Cd_R, via R. Table of Standards	5895.926	—0.014	⎠ & F., spark in vacuo.
P. & F., D_1 direct comparison with Cd_R	5895.932	—0.008	⎞ R. solar, P. & F.
P. & F., D_2 via R. Table of Standards..........	5895.939	—0.001	⎠ flame.
P. & F., Cu 5105 R. Table of Standards..........	5895.953	+0.013	R. solar, P. & F. spark in V.
P. & F., L 6708 R. Table of Standards..........	5895.959	+0.019	R. arc, P. & F. flame.

Average $D_1 = 5895.940$ 0.014 Mean difference.

Comparing this mean value with that assumed by Rowland
from the grating measurements, we have

Rowland (Gratings) $D_1 = 5896.13$
Michelson (Interference) $D_1 = 5895.94$

Difference - - - -   0.19 tenth-meters.

That is, Rowland's wave-lengths must be reduced by very nearly
one thirty-thousandth to bring them into accord with Michel-
son's results. This difference-ratio gives absolute corrections
proportional to the wave-length.

The next question involves the relative experimental errors
of the two methods under comparison. The advantage lies
assuredly on the side of the interference method, but to an
amount which cannot be evaluated until some one repeats
Michelson's work with all possible care. A single set of meas-
urements, even by so skillful an operator as Michelson, cannot
carry concurrent proof of its own precision, and the work,
including even the readings by Benoit, was practically unchecked
save by repetition with no conditions changed. The use of the
three Cd lines furnished material assistance in avoiding errors,
but until the work is independently repeated the precision of the
method is slightly uncertain. Michelson himself (*loc. cit.*)
appreciated this, and in spite of marvelously close agreement

in the figures actually obtained considers that the uncertainty probably may extend to several thousandths of a tenth-meter, *i. e.*, his Cd_R, given as $\lambda_R = 6438.4722$, might be subject to a variation of several units in the next to the last decimal place. Benoit[1] estimates the possible error at about one one-millionth. The work of Perot and Fabry, just discussed, would indicate at least this amount of possible variation, probably extending to more than a unit in the second decimal place. The work of Hamy[2] along the same line shows the possibility of very much larger errors. For Cd_G, given by Michelson as 5085.824, Hamy obtained 5085.903, and, although Hamy found this line triple, its components are so close that the above difference is not adequately explained by this difference of appearance. Bad results are obtainable by any process, so that I am not inclined to attach great weight to this aberrancy. There is common, however, to the results of both Michelson and Perot and Fabry, a source of incertitude which is of serious import.

It has been shown by many researches within the past few years that the vibration periods to which the spectral lines are due are affected by the density of the medium in which the vibration takes place, being quicker *in vacuo* than in free or compressed air or other fluid. More particularly it has been shown by Mohler[3] that under the conditions obtaining in Michelson's vacuum tubes, the apparent wave-length of the *Cd* lines was decreased by 0.014 ± 3 tenth-meters, the corresponding shifts for *Zn* being 0.007 ± 3 tenth-meters, and for *Hg* 0.005 ± 2 tenth-meters. The shift seems to bear for the lines of each element concerned a uniform ratio to the wave-length, but, however this may turn out to be, the net result of the observation is that Michelson's *Cd* wave-lengths, as referred to air at $15°$ and 760 mm, are too short by about 0.01 tenth-meter. The results of Perot and Fabry being referred back to Michelson's Cd_R, obtained *in vacuo*, presumably, and based themselves, except for the *Li* and *Na* lines, on sources *in vacuo*, but under con-

[1] *Rap. Cong. Int. de Phys.*, 1900, **1**, 70.

[2] *C. R.*, **130**, 489, 1900.   [3] ASTROPHYSICAL JOURNAL, **4**, 176, 1896.

ditions bearing an unknown relation to those found in Michelson's *Cd* tubes, are subject to corrections of the same order of magnitude but of unknown amount. This change of vibration period, with the pressure about the radiant, is a very serious matter, especially in certain phases of stellar spectroscopy, and needs a thorough overhauling. Applying the correction indicated to Michelson's *Cd* wave-lengths, the wave-length of D_1 in air at 150 and 760 mm appears to be very nearly 5895.95, as the result of the interference measures thus far made. How far further measures may change this result is a little uncertain, but so masterly was Michelson's original work that it would be rather surprising if the outstanding uncertainty involved more than one or two units in the last decimal place above. In other words, the value of D_1 should certainly fall among the values just tabulated. Its exact location must be slightly dubious until the matter of pressure shifts in the metallic lines involved has been worked out. The work of Huff[1] on Cd_B and of Haschek[2] on various spectra shows that the wave-frequency corresponding to a given line is subject to variation of a very complicated kind, so that comparisons involving solar, spark, and arc spectra, under various conditions, may easily be affected by indeterminate errors amounting to 0.01 tenth-meter or more.

As regards purely experimental errors, grating measurements of absolute wave-length are fairly satisfactory. So far as the probable errors of the experiments with a single grating go, a wave-length determination may be carried on with a high degree of precision. As an example, I may cite my own work with G III (*loc. cit.*). I have fortunately at hand my original records, and deducing the probable error of the comparisons of G III with the standard of reference S_2^a, I find that it amounts to rather less than than one two-millionth without weighting or applying Pierce's criterion. The probable error of the angular determinations similarly treated is 0.00000055, so that the probable error of the result, exclusive of the reference of S_2^a to A_0

[1] ASTROPHYSICAL JOURNAL, **14**, 41, 1901.

[2] ASTROPHYSICAL JOURNAL, **12**, 181, 1900.

(or other ultimate standard of length) amounts to very nearly 0.005 tenth-meters. The results with the other gratings are of about the same precision as regards the angular determinations, but the glass gratings gave somewhat larger probable errors in the length determination. But so far as experimental errors are concerned the result should, from any grating, be in error by less than one five hundred-thousandth. Kurlbaum[1] and Thalén[2] confirm this degree of angular precision so far as their results for a single line and order go, about as theory would indicate from the number of their observations. In other words, with a good modern grating and spectrometer, angular readings should be coherent to 0.01 tenth-meter or less as regards probable error. The measurement of the distance between the terminal lines of n grating spaces, however, is a very different matter. The things to be compared differ greatly in four important particulars: mass, thermal conductivity, coefficient of expansion, and character of the defining lines, each affecting the precision of the measurements in differing degree according to circumstances. The two materials used for gratings are both bad in this respect. Glass is used only for very small gratings, and, while its coefficient of expansion is rather close to that of platinum-alloy and steel standards, it has considerably less than 1 per cent. the conductivity of either, and is transparent besides. Speculum metal, used for the larger gratings, has an expansion coefficient varying slightly in different specimens, but very close to that of brass, than which its conductivity is somewhat less. It therefore is easy to compare with bronze standards, but needs very good temperature control when used with other standards.

The difference in defining lines on various standards is liable to cause material errors. Gratings have beautifully thin, sharp lines compared with which those on most standards look like plough-furrows. This encourages errors of setting, since the coarse lines become ill-defined under any power which permits very precise setting.

[1] *Wied. Ann.* 33, 159, 381, 1887.

[2] *Nova Acta, Ups.* 1899, p. 1. (Date on title of paper itself is 1898.)

As to the total magnitude of the errors in experimental work on grating measurements, there is considerable information available. A good example is found in the work on my standard S_2^2, which resulted in locating a change of length. In this case the comparison by Professor W. A. Rogers with the several double decimeters on his standard R_2 (bronze) and my own determination from a steel standard A_4 which had been compared by Rogers with R_2, differed by only 0.3μ in 200 mm. Benoit [1] thinks that an exactitude of 0.2μ can be guaranteed in a comparison between lengths under favorable conditions, or a little better than that with short standards. The probable error of the Berlin comparison of S_2^2 with the standard R_{78} was 0.15μ, and hence by this comparison the probable error in the length of G III or G IV should not exceed 0.01 tenth-meter when reduced to wave-length.

Now this Berlin standard R_{78} was directly compared with the gratings used by Müller and Kempf [2] and by Kurlbaum. It, therefore, affords a means of directly comparing the results derived from ten gratings used for absolute measurements. Müller and Kempf's gratings were about 20 mm broad, and the probable errors of the length determinations were not far from 0.2μ or about 0.05 tenth-meter in wave-length. Kurlbaum's gratings were 42 mm and 43 mm broad, and the probable error of the length determination is of the order 0.02 tenth-meter.

Collecting the results from the ten gratings in question as referred to R_{78} and applying no corrections thereto except to reduce all to $15°$ C., the wave-length of D_1 is as follows:

M. and K.	"2151"	5896.46
M. and K.	5001	5896.14
M. and K.	8001	5895.97
M. and K.	8001 L	5896.33
Kurlbaum	I	5895.84
Kurlbaum	II	5895.96
Bell	I	5896.24
Bell	II	5896.18
Bell	III	5896.32
Bell	IV	5896.16

Mean $= 5896.160$, or omitting "2151," $= 5896.126$

[1] *Rap. Cong. Int. de Phys.*, 1900, 1, 66. [2] *Publ. Potsdam Observ.*, V, 1886.

Between the extremes there is a difference of no less than 0.62 tenth-meter, almost one ten-thousandth ! Casting out " 2151 " entirely as exceptionally bad and recognized so to be, and making a liberal allowance for the probable errors just examined, there still remains an outstanding variation of more than 0.3 tenth-meter which cannot by any reasonable process be charged to experimental errors of any kind. It represents, in fact, the magnitude of the errors in ruling, the difference between the mean grating space as measured and the grating space optically effective in producing the spectra upon which the angular measurements are made. The analysis of the error is worth following up.

In general, the ruling of a grating is not uniform, and a grating of total breadth B, instead of having n spaces, each of $1\,\mu$ in width will have

$$B = o\,(b\mu) + p\,(c\mu) + q\,(d\mu) + \cdots \cdots .$$

One compares his standard of length with the first member of this equation, but obtains his spectra from the gratings defined by the second member. In practice there is one greatly predominant grating space, say $b\mu$, o being a very large fraction of n.

The theory of errors in ruling gratings has been investigated rather fully by Rayleigh,[1] Rowland,[2] Cornu,[3] and others, but there is much relating to the effect of these errors on absolute and relative wave-length measurements which does not at once appear from the equations. For ordinary ocular observations the chief practical requirement is that the errors, whatever they may be, shall not perceptibly injure the general definition or produce visible false lines, assuming the full aperture of the grating to be in use. This merely demands that the term in $(b\mu)$ above shall predominate so far as to drown out the spectra . due to the other grating spaces.

This condition may coexist with very serious errors, which would appear on thorough investigation of the grating. It is these optically ineffective errors which cause the most serious trouble in spectrometry. A grating in which the errors were so large and so distributed as to give blurred definition, false lines,

[1] *Enc. Brit.*, **24**, 437 *et seq.*   [2] *A. and A.*, **12**, 129, 1893.   [3] *C. R.*, **116**, 1421, 1893.

and large variations in focus would not be used in these days for any delicate or important work.

The commonest error in ruling is a simple periodic variation in the grating space, due to eccentricity of the screw-head, periodic errors in the screw itself, or bad bearings. Its period is generally the revolution of the screw and the amplitude of the fluctuation in grating space is usually rather small in carefully ruled gratings, so that the definition is seldom perceptibly injured. The well-known effect of this particular error is to produce "ghosts" and to diffuse a little light (Rowland, *loc. cit.*).

In good modern gratings ghosts are inconspicuous save now and then in the high orders, and they are from their symmetrical position so easily distinguished as to give little trouble to the experienced spectroscopist. In photographing metallic, or other bright line spectra, ghosts may commonly be detecte deven in gratings giving the most superb definition. The ultra-violet *Mg* lines are most effective ghost-raisers[1], and the grating that will not respond to them on prolonged exposure is a treasure. So far as measurements are concerned the ordinary periodic errors are not serious if B is, as usual, an integral number of periods.

An error almost equally common is the simple linear error, usually chargeable to variations of temperature. If considerable in magnitude and extent it ruins the definition and throws the spectra on the two sides widely out of focus. The latter effect may exist even when the definition is good, and has been investigated by Cornu (*loc. cit.*). The danger of this particular error lies in the fact that it seldom affects the whole ruled surface and commonly appears at one end only. For instance in a grating 10 cm broad the error may only affect 1 cm or less to a perceptible degree. It may be found by going over the grating with a slit a few millimeters wide and looking for displacement of a spectral line. It is rarely perceptible when the full aperture is used, either in the definition or focus, and has to be looked for carefully; when small in extent it does not affect the principal spectrum at all but is, of course, included in measuring B and

[1] Especially the one at $\lambda 2852$.

may lead to a serious error in absolute measurements. Once located the extent of the error introduced in B may be found by the process of micrometrical calibration which I used, or by computation from the change in focus, following Cornu. Either process will enable the greater part of it to be eliminated, but a residual error due to minor variations of the same kind of too small extent to be readily eradicated is likely to remain unless in exceptionally good gratings. The definite errors in B which I found produced by such errors in the grating space amounted in one grating to 0.45μ in 10 cm.

These common abnormalities must be watched for in photographic work, for they may easily give rise to false lines, particularly in the higher orders where the increased dispersion leads one to look for additional lines. False lines due to this cause can of course be eliminated by stopping successive portions of the grating, beginning at the ends, which should first be under suspicion. Relative measurements with the concave grating can hardly suffer much from errors of this kind unless in using the method of coincidences a line of the higher order is shaded off by such a false line so as apparently to displace it.

In using closely ruled gratings, wherein space and groove do not differ greatly in width, spectra of faint orders may suffer especially from false lines, since the extinguishment factor will not be the same for the normal and the aberrant grating spaces so that one may confuse the real and false lines, or they may so run together as to give no sign of trouble in the character of the resultant line. In gratings such as one would use for careful measurements these phenomena would very rarely rise to material amounts, and the apparant shifting would almost universally be far within the small micrometric errors.

Far graver are the single or recurrent, non-linear errors such as the one which I found in grating III[1], used for my wavelength measurements. This instance was the appearance, in a grating otherwise of wonderful uniformity so far as could be detected, of a group of lines of spacing widely abnormal, involving

[1] *Am. Jour. Sci.* (3) **35**, 361, 1888.

an error of 2.5μ in B within the space of less than one hundred lines. Only about twenty lines were concerned in the worst of the variation so that it is well within bounds to say that the grating space at this point was aberrant by about 10 per cent. The lines too were here scored deep and rough. The calibration centimeter by centimeter showed only trifling errors elsewhere.

Errors of this kind and magnitude are rare, but cannot be detected by anything save the microscope and not always even so. In this instance the slit test showed nothing, and while it is possible that a photograph of a bright line spectrum with long exposure might have given a false image it would have been so wide from the real line that the connection would not have been apparent.

I am inclined to think that in errors of this kind the discrepancy between grating and interference measurements has its source. A very small recurrent group of slightly aberrant grating spaces is amply sufficient to account for the whole observed difference. Suppose for example that in a grating 10 cm broad and ruled at $s=0.0025$ mm, like my grating IV (*loc. cit.*), there were in each millimeter a group of ten lines spaced even so little as 0.0025μ too widely. Then each group would represent an error of 0.025μ in B and the accumulated error would be one forty-thousandth in the resulting wave-length.

But neither the slit test, calibration, or difference in focus would give the slightest clue to such an error. Photography would show the existence of a special case of ghosts but could not lead one back to an error beyond the reach of the micrometer.

The requirement for such a fault is a recurrent aberrance of a few lines in the same sense, periodic as regards the engine but non-sinusoidal, so that the effect is cumulative as regards B. The spectra from the main grating space alone are visible but B is increased by a relatively considerable quantity. Such an error could be produced by a hard streak along the axis of the screw, bearing the merest trifle unequally upon the nut perhaps even in passing a split, or by a very minute inequality in the thrust bearing.

Even the accidental recurrence of errors enormously less than
that found in grating III would produce a large error in B, but
they would hardly persist through many gratings from the same
engine.   It is sufficient to call attention to this type of error, not
reckoned with in the ordinary consideration of gratings, but
easily sufficient to render gratings useless for absolute measure-
ments.

Some new light is thrown on grating measurements by the
research of Thalén,[1] who worked with a Rowland grating which
was measured before and after the angular measurements, at the
International Bureau of Weights and Measures.   Thalén found
for D_1 the value 5895.946, which agrees with the interference
measurements in a most striking manner.   But since the two
measures of Thalén's grating made three years apart differ by
very nearly one five hundred-thousandth, this extreme precision
must be considered more apparent than real.   One might naturally
infer that Thalén was fortunate enough to obtain a grating prac-
tically without errors.   His measurements however are in doubt
by an amount considerably greater than the above value for D_1
would indicate, since, as Kayser[2] points out, he found in measur-
ing various lines in absolute measure a nearly constant difference
from Rowland's values quite irrespective of wave-length, while
obviously he should have found a constant *ratio*.   The unusual
feature of Thalén's research was his extension of the angular
measures over a very wide temperature range, thus concurrently
determining both the deviation and the temperature coefficient
of the grating.   This innovation is of dubious usefulness, since
his value for E from all the measures was 5270.315, while reduc-
ing the observations near 15° by the coefficient of expansion
deduced from other lines observed at widely different temper-
atures gave $E = 5270.378$.

Considering now the wide discrepancies found in wave-
length measurements referred to the same standard of length,
the relatively high precision of the angular measurements
involved, the known and great effect of errors in ruling and the

[1] *Nov. Act. Ups.*, 1898.   [2] *Handbuch der Spectroscopie*, 1, 707.

probability of the particular kind of error to which I have here drawn attention, I am forced to the conclusion that the diffraction grating is, and is likely to remain, entirely insufficient for the determination of absolute wave-length.

So far as relative measurements are concerned the method of coincidences is so precise as to leave little to be desired. Such possible discrepancies as may exist in Rowland's results, are with rare and dubious exceptions, well within the experimental errors of any method used for checking them. Rowland's values were so thoroughly cross-referenced that there was small chance left for large accidental errors.

In a most interesting recent paper Perot and Fabry[1] have instituted a direct comparison between certain solar lines and the green *Cd* ray at λ 5086. From their interference measurements they have derived an apparent systematic error in Rowland's tables, expressed in an irregular curve already published elsewhere.[2] The variations from a uniform ratio are rather small, in the neighborhood of 0.01 tenth-meter for the most part, and the curve evidently contains the summed errors of Rowland's and their own investigations.

As the solar lines examined do not correspond with any fundamental lines determined by interference methods it is difficult to obtain any check on the results. One can be derived, however, through Cd_B measured by Michelson, and *Zn* λ 4810 measured by Perot and Fabry themselves.

It happens that these two lines when reduced through Rowland's table of standards lead to the same value for D_1 also, of course, showing exact agreement as to their relative wave-lengths with Rowland's table.

Now, if Perot and Fabry's curve of ratios represents the facts, its value at these wave-lengths, applied to the wave-lengths found for the two lines mentioned, ought to lead directly back again to Rowland's values for the same bright lines.

In other words, if Rowland's values for this pair of lines are correct as regards relative wave-lengths, then Perot and Fabry's

[1] *Ann. de Ch. et de Phys.*, January 1902, p. 98.   [2] *C. R.*, **133,** 154.

ratio determined with respect to the solar tables on which these two wave-lengths are based, must apply, provided Perot and Fabry introduced no error in passing from their green cadmium line to their solar wave-lengths. If Rowland's relative values for this pair are wrong, then the relative values of Perot and Fabry, and Michelson for the same lines, must also be wrong, since they agree with Rowland's.

In point of fact the ratio in question leads to a value 0.02 tenth-meter too small for Rowland's lines. Two other lines of Zn, λ 4680 and 4722, lead to almost exactly the same discrepancy.

Of course, the question of pressure shifts as between the arc in air and the spark at reduced pressure enters any such comparison, but if it should prove an adequate explanation it would furnish proof conclusive that wave-lengths cannot safely be carried to a higher degree of precision than just indicated without exact knowledge of and correction for the pressure shift in the source used.

Another check on the validity of these corrections lies in the comparison between Perot and Fabry's values for certain bright iron lines and the same lines reversed in the same solar spectrum. From their tables,[1] one can cull nine pairs of corresponding lines. In these the shift of the solar lines varies from $+0.019$ tenth-meter to -0.019 tenth-meter, again denoting errors or physical differences demanding explanation, of about the same order of magnitude as those just recorded and those indicated in the table of values for D_1 previously given.

Finally reference must again be made to the wide discrepancy between the results of Hamy and others for the green Cd line λ 5086. This amounts to 0.079 tenth-meter. If differences in the source, or in the appearance of the line in the interferometer are competent to produce a difference like this, then Cd_G is a very shaky sort of standard. It is worth noting that the reversal of this line in the Sun, if Rowland's tentative line is really a reversal, corresponds very closely to Hamy's value for the head line of

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[1] *Ann. de Ch. et de Phys.*, January 1902.

his triplet. A similar condition has been noted by Rówland[1] in the case of some of the *Mg* lines.

In view of these facts Perot and Fabry's corrections to Rowland's tables seem to be based on inadequate evidence. There are doubtless plenty of minute errors in this table but it has yet to be shown that the interference methods are sufficiently developed to evaluate them with certainty.

To clear up the wave-length matter we need most of all an entirely independent determination by Michelson's method to show its working limits of error; second, a very thorough study of pressure shifts to define standard working conditions; and finally, a careful study of the constancy of wave-length in the solar lines, following up Jewell's admirable preliminary paper.[2]

For absolute measurements the grating must now be considered as out of the game. For relative measurements it has less equivalent dispersion but greater brilliancy and sharpness of definition than the interference methods. Personally I do not think that the available evidence points to a greater real precision than about 0.01 tenth-meter for either method in the present state of the art. At about this point one reaches a dubious ground among pressure shifts and similar small corrections where the values become very uncertain. To the interpretation of these phenomena much future work must be addressed, for they may furnish the key to the great problems of molecular mechanics.

BROOKLINE, MASS.,
    March 14, 1902.

[1] *A. and A.*, **12,** 321 *et seq.*   [2] ASTROPHYSICAL JOURNAL, **3,** 89, 1896.

THE APPARATUS FOR THE ELECTRIC HEATING OF THE POTSDAM SPECTROGRAPH, NO. III.[1]

By J. HARTMANN.

THE effects of changes of temperature become very disturbing in the case of all accurate measurements made with prism spectroscopes. This is particularly the case in the photography of stellar spectra, for then the spectrograph attached to the refractor is exposed to all the variations in the air temperature in the well-ventilated dome during exposures commonly of an hour or more. The first plates of this kind, taken by Vogel and Scheiner in 1888 for determining the velocity of stars in the line of sight, indicated the necessity of paying particular attention to the temperature changes, and Professor Vogel gives in the *Publicationen des Astrophysikalischen Observatorium* (**7,** 24, 1892), a precise statement of all the phenomena coming into question.

The effect of a change in the temperature during an exposure is in the first place due to the change in the index of the prisms, and in the second to the thermal expansion of the metallic portions of the apparatus. If the temperature of the prisms rises, the deviation increases, and the spectrum is displaced on the plate in the direction of shorter wave-lengths. The amount of this displacement is only slightly altered by the expansion of the metallic parts. If such a change of temperature occurs during the exposure of a star spectrum, the lines will not only become diffuse, but they may also be displaced with respect to a comparison spectrum taken on the same plate, causing a systematic error in the determination of the star's velocity. To avoid this error Professor Vogel then arranged to give to the comparison spectrum a number of short exposures

distributed uniformly through the whole exposure to the star spectrum. All the displacements undergone by the star spectrum during the exposure must thus be transferred to the comparison spectrum, and although the lines of both spectra would now appear somewhat lacking in sharpness, there would be no danger of a systematic error in the relative positions. That this arrangement was effective in greatly diminishing the influence of temperature variations is best proven by the accuracy of the results then obtained. It did turn out to be necessary to limit the exposure to about an hour, since the increasing broadening of the lines with longer exposures made accurate measurements more and more impossible.

The displacement of the spectrum due to the variations of temperature in one evening is so considerable that it may be several times as large as that due to the motion of the star. The displacement of the spectrum per degree of change of temperature can be computed from the values of the temperature coefficient of the indices of the different kinds of glass, as determined by Müller,[1] Pulfrich,[2] and others.

The following table gives the displacement $d\lambda$ for $1°$ C. for the lines $H\gamma$ and D with the three flint prisms employed by Müller, and with compound prism No. 6. v is the velocity of a star which would produce an equal displacement of the lines:

TABLE I.

Displacement of lines for $1°$ C. change of temperature.

Prism.	$H\gamma$, $\lambda = 4341$		D, $\lambda = 5893$	
	$d\lambda$	v	$d\lambda$	v
Flint No. 1...............	0.28 t. m.	19.4 km.	0.52 t. m.	26.5 km.
Flint No. 2...............	0.37	25.4	0.68	34.6
Flint No. 3...............	0.29	20.2	0.42	21.4
Compound No. 6..........	0.35	24.2	0.73	37.2

[1] G. MÜLLER, "Ueber den Einfluss der Temperatur auf die Brechung des Lichtes in einigen Glassorten, im Kalkspath und Bergkrystall," *Publ. des Astroph. Obs.*, **4**, 149, 1885.

[2] C. PULFRICH, "Ueber den Einfluss der Temperatur auf die Lichtbrechung des Glases," *Wied. Ann.*, **45**, 609, 1892.

The table shows that very large displacements occur, and that they vary greatly for the different kinds of glass. The conditions are much more favorable for crown glass, for the displacements are considerably less, and, indeed, become zero at a point near D, changing their direction beyond that point toward the red. The displacement for quartz is nearly the same as for flint glass but with opposite sign. If it was desired to construct a spectroscope as insensitive as possible to changes of temperature, prisms of crown glass or a combination of quartz and flint prisms could be employed.

The figures in the table show only that part of the displacement which is produced by the change of index of the glass. As the position of the spectrum in a compound spectroscope is also affected by the expansion of the metallic parts, it would be of interest to investigate the effect of temperature changes on the completed spectrograph. To this end I have made the following experiment with four spectroscopes. After a spectrograph had stood for a long time in a room of uniform temperature, an iron spectrum was photographed. The room was then heated, and after the instrument had again attained, some hours later, a steady temperature about 15° higher than before, a second iron spectrum was taken on the same plate, which had meanwhile remained entirely undisturbed. The experiment was then repeated with a decreasing temperature. The results are collected in Table II. There is one flint prism of about 60° angle in spectrographs *D* and No I, three in No. III, and two compound prisms in the older instrument *A*, with which Vogel and Scheiner made their plates in the years 1888 to 1890.

These measured displacements deviate considerably from the figures given in Table I, as was to be expected in view of the difference of the kinds of glass and the concurrent effect of the metallic parts. They are of the same order of magnitude for instruments *D*, *A*, and III, and only spectrograph I exhibits a different behavior. For this the displacement is wholly neutralized at $\lambda 4252$ by the expansion of the metallic parts; and the rest of the spectrum is only slightly affected by changes of tem-

perature. This compensation does not occur so favorably in case of rapid changes, as the metallic parts will not always have just the temperature of the prisms.

TABLE II.

Displacement of lines for $1°$ C. change of temperature.

λ	Spectrograph D		Spectrograph I		Spectrograph III		Spectrograph A	
	t. m.	km.	t. m.	km.	t. m.	km.	t. m.	km.
3800	.....	....	+0.057	+4.5	.....	....	.....	....
3900	.....	....	+0.046	+3.5	.....	....	.....	....
4000	.....	....	+0.035	+2.6	.....	....	.....	....
4100	+0.251	+18.3	+0.023	+1.7	+0.212	+15.5	+0.233	+17.0
4200	+0.265	+18.9	+0.009	+0.6	+0.215	+15.4	+0.241	+17.2
4300	+0.279	+19.4	−0.008	−0.6	+0.217	+15.2	+0.250	+17.4
4400	+0.293	+20.0	−0.024	−1.6	+0.220	+15.0	+0.259	+17.6
4500	+0.307	+20.5	−0.041	−2.7	+0.222	+14.8	+0.267	+17.8
4600	.....	....	−0.059	−3.8	.....	....	.....	....
4700	.....	....	−0.076	−4.8	.....	....	.....	....
4800	.....	....	−0.095	−5.9	.....	....	.....	....
4900	.....	....	−0.114	−7.0	.....	....	.....	....
5000	.....	....	−0.134	−8.0	.....	....	.....	....

In the course of these experiments I tried to answer a second question, viz.: In what way and how soon are the prisms affected by the variations of the external temperature. On another occasion[1] I derived certain theorems bearing on the behavior of bodies not in thermal equilibrium. If Newton's law of cooling holds for such a body, this differential equation will be true :

$$\frac{dA}{dt} = \gamma (A - W), \tag{1}$$

where A is the temperature of the body and W that of the surrounding air, and γ is a constant characteristic of the temperature sensitiveness of the body. By integration we get

$$\frac{\log (A - W) - \log (A_0 - W)}{t - t_0} = M\gamma, \tag{2}$$

where M is the number $0.43429. \ldots$ We may use equation (2) to test whether or not a body follows the above simple law

[1] *Zeitschrift für Instrumentenkunde,* **17**, 14, 131, 1897.

in its changes of temperature. For this purpose we bring the body, at another temperature, into a space at the constant air temperature W, and then we determine the temperature A of the body at suitable intervals. The law of cooling is fulfilled if we always get the same value of the product $M\gamma$ in (2) from all these observations.

In order to render possible the measurement of the momentary temperature of the prisms, I have utilized the change of the dispersion itself as a criterion, since it is so large that the temperature of the prisms can be determined to within a few tenths of a degree by it. The experiments were performed as follows : Spectrograph III was first left for a long time in a space at the constant temperature of $+15°.8$ C., and when it was certain that all parts of the apparatus had assumed this temperature, four plates of the iron spectrum were made (Nos. 1 to 4 in Table III). Then, at the time t_0, the instrument was carried into an adjoining room, which was maintained by heating at a constant temperature of $+26°.0$. Another plate was taken after 20 minutes, and again after 40 minutes, and 1, 2, 3½, and 6 hours. Nineteen hours later, when the instrument had fully assumed the new temperature, plates 11 and 12 were made, and after 24 hours plates 13 to 16. The distance D between the two

TABLE III.

Exposure	Plate No.	$t - t_0$ minutes	θ	D	A	$M\gamma$	A'	$O. - C.$
1......	III 396	....	$+15°.8$	65.196	$+15.2$	....	$+15°.8$	$-0°.6$
2......	397	....	15.8	217	16.3	....	15.8	$+0.5$
3......	402	....	15.8	209	15.9	....	15.8	$+0.1$
4......	403	....	15.8	210	15.9	....	15.8	$+0.1$
5......	396	20	20.7	237	17.3	-0.00338	18.0	-0.7
6......	397	40	22.6	293	20.1	-0.00594	19.7	$+0.4$
7......	398	60	23.8	313	21.1	-0.00531	21.0	$+0.1$
8......	399	120	25.4	368	23.9	-0.00569	23.6	$+0.3$
9......	400	210	25.9	392	25.1	-0.00500	25.2	-0.1
10......	401	360	26.0	411	26.0	....	25.9	$+0.1$
11......	399	1140	25.8	403	25.6	....	25.8	-0.2
12......	402	1140	25.8	408	25.9	....	25.8	$+0.1$
13	398	1440	26.8	423	26.7	....	26.8	-0.1
14......	400	1440	26.8	418	26.4	....	26.8	-0.4
15......	401	1440	26.8	438	27.4	....	26.8	$+0.6$
16......	403	1440	26.8	428	26.9	....	26.8	$+0.1$

iron lines at λ 4202 and 4495 was measured on all the plates. The reduction of this series of observations is contained in Table III, in which θ denotes the reading of thermometer belonging to the spectrograph, its bulb in the interior of the prism-box. (I shall again advert to the behavior of this thermometer.) D is the measured distance between the two iron lines, expressed in revolutions of the micrometer screw of 0.5 mm pitch, and A is the prism temperature computed therefrom.

The computed values were obtained as follows: We get from exposures

$$1\text{--}4 \qquad D = 65.2080 \text{ revs. at } A = 15°.8$$
$$11 \text{ and } 12 \qquad 65.4055 \qquad\qquad 25.8$$
$$13\text{--}16 \qquad 65.4268 \qquad\qquad 26.8$$

Hence for $1°$ rise of temperature we get a change of 0.01982 revs. in D, whence follow the values of A. The values of the product $M\gamma$ were computed by (2) from exposures 5 to 9, made during the rapid change of temperature. There is no marked progressive change in these figures, and only the first one differs seriously from the others, in consequence of some accidental disturbance. As the mean of the whole series, we get

$$M\gamma = -\,0.005238,$$

from which again by (2) the theoretical temperatures A' are calculated, which the prisms would have had if they had more closely followed the law of cooling. The values of $O.\text{---}C.$, or $A\text{---}A'$, in the last column, show that the prism temperature is exceedingly well represented by these computed values.

A second series of observations was made with falling temperatures, in order to fully assure this result. The results of the measurements, which were of the distance between the lines $\lambda 4228$ and λ 4529, are given in Table IV.

The separate values of $M\gamma$ show again an excellent agreement, the mean being

$$M\gamma = -\,0.00420,$$

with which value the calculated temperature A' represented the observed prism temperatures within the errors of observation.

It is not surprising that the numerical value of $M\gamma$ differs somewhat from that found from the first series, since the value of these constants is in a high degree dependent upon the motion

TABLE IV.

Exposure	Plate	$t-t_0$ minutes	θ	D	A	$M\gamma$	A'	$O.-C.$
1	III 404	..	$+26°8$	50.405 revs	$+27°0$		26°8	$+0°2$
2	405	..	26.8	399	26.6		26.8	-0.2
3	406	15	23.4	382	25.5	-0.00379	25.3	$+0.2$
4	407	30	21.7	371	24.7	308	24.1	$+0.6$
5	408	45	20.7	351	23.4	368	23.0	$+0.4$
6	409	60	19.8	337	22.4	375	22.0	$+0.4$
7	410	90	18.3	307	20.4	433	20.5	-0.1
8	411	120	17.5	280	18.6	519	19.4	-0.8
9	412	180	16.7	263	17.4	489	17.9	-0.5
10	413	240	16.3	265	17.6	350	17.1	$+0.5$
11	414	900	15.7	236	15.6		15.7	-0.1
12	415	1440	15.7	240	15.9		15.7	$+0.2$

of the surrounding air, as I have shown elsewhere. I take as mean of the two series

$$M\gamma = -0.0047.$$

Inasmuch as the temperature of the prisms follows very closely the law of cooling, as shown by these series of observations, we may now employ the theorems I have proposed in order to obtain an accurate conception as to the course of the variation of the prism temperature. Special interest would be attached to the answer to these two questions: How does the prism temperature change (1) with a sudden change of the external temperature, (2) with a uniform change of the air temperature? The first case occurs when the instrument is brought from a cold into a warm room and is to be used in the latter, or if the dome is opened and aired before beginning exposures of stellar spectra in the evening. A nearly uniform fall of the air temperature will then commonly occur during the exposures. The reply to the first question is contained in Table V, which is calculated with $M\gamma = -0.0047$ in equation (2). $A-W$ is as before the difference in the temperature of prisms and air; t is the corresponding time.

As an illustration of the use of the table, suppose the air temperature quickly fell 4° after opening the dome; then the value of t corresponding to this $A - W$ would be approximately $2^h 30^m$. The table then teaches that after another hour, at $t = 3^h 30^m$, the prism temperature will be 2° higher than that of the external air; and after two hours will still be 1° higher. In round numbers we may say that the difference between the temperature of the prisms and of the outer air decreases by one half per hour (more precisely in 64 minutes).

TABLE V.

$A - W$	t	$A - W$	t	$A - W$	t
	h m		h m		h m
20°.00	0 00	6°.77	1 40	0°.56	5 30
18.07	0 10	5.46	2 00	0.41	6 00
16.11	0 20	3.94	2 30	0.21	7 00
14.45	0 30	2.85	3 00	0.11	8 00
12.97	0 40	2.06	3 30	0.06	9 00
11.64	0 50	1.49	4 00	0.03	10 00
10.45	1 00	1.08	4 30	0.01	12 00
8.41	1 20	0.78	5 00	0.00	18 00

The second question as to the change of the prism temperature with variable air temperature is answered by formula 4, on p. 15 of my paper already referred to, viz.,

$$\log (W_1 - x) = M\gamma t + \log (W_1 - x_0).$$

Here $x = \gamma (A - W_0 - W_1 t)$; the subscript 0 is given to the values for $t = 0$, and W_1 is the change in the air temperature in one minute. To illustrate the use of this formula I compute the following example:

Let the air temperature fall rapidly from $+15°$ to $+10°$ after opening the dome, and thereafter fall $1°.2$ hourly, or $0°.02$ per minute. Then we have

$$A_0 = +15° \qquad W_0 = +10° \qquad W_1 = -0°.02$$
$$M\gamma = -0.00470 \qquad \gamma = -0.0108$$
$$x_0 = 5\gamma = -0.0540 \qquad W_1 - x_0 = +0.0340.$$

Table VI shows the resulting course of change of the temperatures.

TABLE VI.

t	Temperature of air, W	Temperature of prisms, A	$A - W$
h m			
0 00	$+10°.00$	$+15°.00$	$+5.00$
0 30	$+\ 9.40$	$+13.53$	$+4.13$
1 00	$+\ 8.80$	$+12.30$	$+3.50$
1 30	$+\ 8.20$	$+11.22$	$+3.02$
2 00	$+\ 7.60$	$+10.31$	$+2.71$
3 00	$+\ 6.40$	$+\ 8.70$	$+2.30$
4 00	$+\ 5.20$	$+\ 7.29$	$+2.09$
5 00	$+\ 4.00$	$+\ 5.97$	$+1.97$
6 00	$+\ 2.80$	$+\ 4.72$	$+1.92$
8 00	$+\ 0.40$	$+\ 2.27$	$+1.87$
10 00	$-\ 2.00$	$-\ 0.14$	$+1.86$
12 00	$-\ 4.40$	$-\ 2.55$	$+1.85$

We see that under the assumed conditions, which might easily occur in observing practice, the prism temperature changes $2°.7$ in the first hour and $2°$ in the second hour. This change, moreover, occurs in such a way, as I showed previously, that the difference $A - W$ approaches a fixed limit, in this case $1°.85$.

The readings θ of the thermometer attached to the spectrograph by no means indicate the true temperature of the prisms during a rapid fall, as is clearly shown in Tables III and IV; in Table III the difference $\theta - A'$ is as large as $2°.9$, in Table IV as $2°.4$. As was to be expected, the readings of this thermometer do *not* follow the law of cooling.

These rapid changes of the prism temperature cannot, of course, take place uniformly throughout the entire mass of each prism, but for rising temperature the heat will first warm the thinner parts of the glass—near the edges—and the interior of the prism will not attain this temperature until later. Meanwhile, the prisms are not optically homogeneous, as is clearly shown by the form of the lines of the spectrum. On plates 5, 6, and 7 of Table III, made at rising temperature, there is a faint shading on the violet side of every line; but on plates 3, 4, and 5 of Table IV, taken with falling temperature, these shadings lie, *per contra*, on the side toward red. Inasmuch as the shading always

lies on the side toward which the whole spectrum is moving on
account of the temperature change, it follows that the portion
of the prism which produced this diffuse shading has taken on
the new temperature sooner than the rest of the mass of glass;
and this portion is evidently the thinnest part, near the refract-
ing edge. It must, however, be pointed out that this deteriora-
tion in the sharpness of the lines from the thermal strain of the
prisms is not very large, even in case of the very strong changes
of temperature given above — the lines can still be set upon
quite accurately. The loss of sharpness due to the gradual
shifting of the whole spectrum is incomparably larger.

In view of what has been said, no further doubt can remain
that the prism temperature must be maintained constant for all
plates of stellar spectra which are to be accurately measured.
Several astrophysicists have accordingly provided their spectro-
graphs with heating apparatus. Wrapping the instrument in
poor conductors, such as felt or woolen covers, can only delay
the escape of heat; with a long continued uniform fall in the
temperature of the surrounding air, such a wrapping will indeed
be entirely useless, for, as appears from Table VI, the change of
temperature will proceed at the same rate both within and with-
out.

Deslandres [1] was the first to introduce the artificial heating
of a spectrograph, at the Meudon Observatory. He surrounded
the whole apparatus with a strong metallic case with double
walls, between which constantly flowed a stream of water from
the city mains. Since the temperature of this water was pretty
constant, he thus succeeded in maintaining the temperature of
the prisms at nearly the same point for days at a time. In spite
of the simplicity of this arrangement, it is hardly to be recom-
mended, as it greatly increases the weight of the apparatus,
and does not insure the perfect constancy of the temperature.

Lord [2] provided the stellar spectrograph, of the McMillin
Observatory, at Columbus, with electric heating. Spirals of

[1] *Bulletin Astronomique*, 15, 49, 1898.

[2] ASTROPHYSICAL JOURNAL, 8, 65, 1898.

wire, which could be heated by a current of about six amperes, were attached at suitable places on the metallic frame of the apparatus ; the whole is wrapped in a double layer of felt, through which projected a thermometer, the bulb of which was within the prism-box. The observer reads the thermometer from time to time, and turns on the current for a short interval as soon as a fall in the temperature is noticed.

The heating apparatus applied by Campbell to the Mills spectrograph of the Lick Observatory is quite similar, except that he made the important improvement of placing the bulb of the thermometer outside instead of inside of the prism-box, and in the large wooden box which covered the whole spectrograph. In this way the observer can correct for any small change of temperature occurring *outside* of the prism-box by promptly turning on the current. Careful observation of the control thermometer made it possible to maintain the temperature inside the prism-box within $0°.1$ C. Inasmuch as Campbell employs only a single thermometer attached to one wall of the felt-protected outer box, for determining its temperature, there is a danger of a stratification in the temperature within the box during the exposure of each star. The thermometer in the prism-box would then give a constant reading during the exposure to the particular star ; but on pointing to another part of the sky, with the spectrograph in another position, the prism temperature might change from the effect of another air stratum, even if the readings of the outer thermometer were kept constant by heating.

In order to avoid taxing the observer's attention too much with the temperature control as it is sufficiently occupied with accurately holding the star on the slit of the spectrograph, I have arranged the heating for the new spectrograph (No. III) of the Potsdam Observatory so that the temperature of the air surrounding the whole instrument is automatically kept constant. The kindness of Professor Hagen gave me an opportunity of becoming acquainted at the *Reichsanstalt* with the different kinds of thermostats which have proven to be practical. The device

made according to my specifications by Mr. Toepfer, of Potsdam, is shown in Figs. 1 and 2.

Professor Vogel has given a precise description of the spectrograph in the ASTROPHYSICAL JOURNAL (**11,** 393, 1900). As this paper may be inaccessible to many readers of the *Zeitschrift für Instrumentenkunde*, I will first briefly describe the spectrograph.
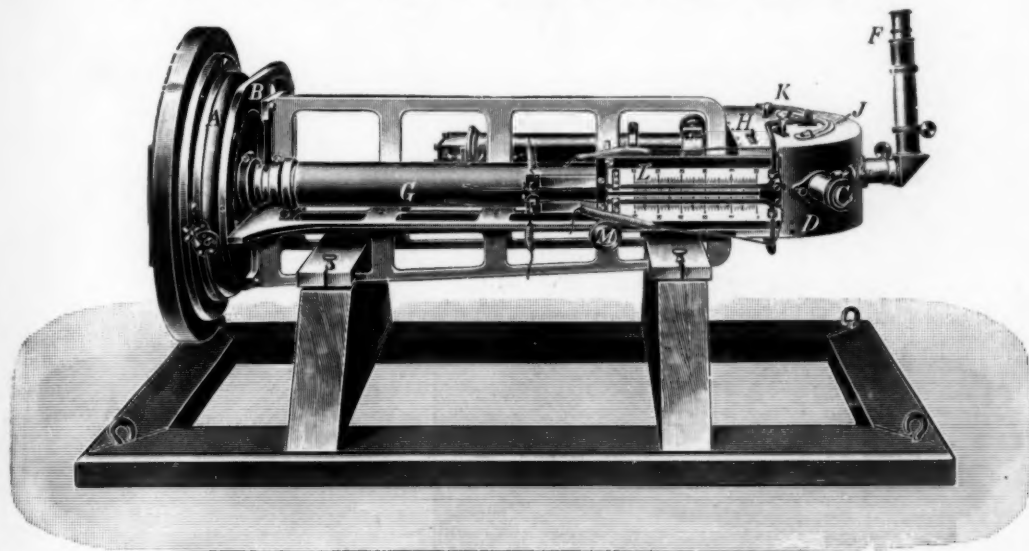
FIG. 1.

Fig. 1 shows it mounted on the wooden support as it is used for plates made in the laboratory. It is attached to the adapter of the 80 cm refractor by the strong cast-iron plate *A*, as shown in Fig. 2. An iron plate *B*, 10 mm thick, is firmly screwed to the plate *A*, and itself supports a very rigid truss-work of iron plates. To this the optical parts are attached. *G* is the collimator, of 32 mm aperture and 48 cm focus. The prism-box *D* contains three excellent prisms of the heavy silicate flint O 102 of Schott & Co., furnished by Steinheil. Their refracting angle of 63° 27′ is so chosen as to make the total deviation for *H*γ accurately 180°, and the prisms are rigidly set for minimum deviation for *H*γ. The camera *E* has an objective of 40 mm

aperture and 41 cm focus; it can be rotated about an axis close in front of the last prism surface, and can be rigidly screwed to the base plate in five different positions, in which the rays D, F, $H\gamma$, $H\delta$, and K successively fall at the center of the plate. This camera can also be wholly removed and a longer one of 42 mm aperture and 56 cm focus can be substituted. The guiding telescope F receives the light reflected from the first surface of the second prism, after it has been dispersed by the first prism, and furnishes a permanent control on the accurate position of the star in the slit. For faint stars this telescope can be slipped into the tube C and then the image of the slit as directly reflected from the first prism surface is observed. At H there is a small slit through which may be slid the various diaphragms used in testing and focusing the apparatus. The whole prism-box consists of stiff sheet nickel and offers an excellent protection against radiant heat. The bulb of the thermometer J, the readings of which were designated above by θ, is situated directly under the surface of the prism-box.

For controlling the air temperature outside of the prism-box, two mercury contact thermometers are attached, with their long semicircular bulbs at a distance of 2 cm from the two base surfaces of the prism-box. The upper bulb is visible at K. The capillaries of the thermometers are led after several bends to the side of the collimator, where they are attached to their scales L parallel to each other. Platinum wires are melted into the glass just below the ends of the scales, making contact with the mercury column. Two platinum wires of 0.3 mm diameter are introduced at the open ends of the capillaries, which are of 0.5 mm diameter, and can be set by the rack and pinion M at any desired points on the scales.

If the spectrograph is to be used for stellar spectra it is inclosed in the light wooden box seen in Fig. 2, which is attached by six long screws to the edge of the plate B, but otherwise does not touch the spectrograph. Two electric heaters are attached on the inside of this box at points opposite to the semicircular bases of the prism-box. One of these heaters is

visible on the opened door in Fig. 2. It consists of a wooden frame *OO* carrying two glass rods, between which a length of 20 meters of German-silver wire of 0.24 mm diameter makes a large number of turns. Spiral springs keep the wires taut at all temperatures. The frame is attached at a distance of 1 cm from
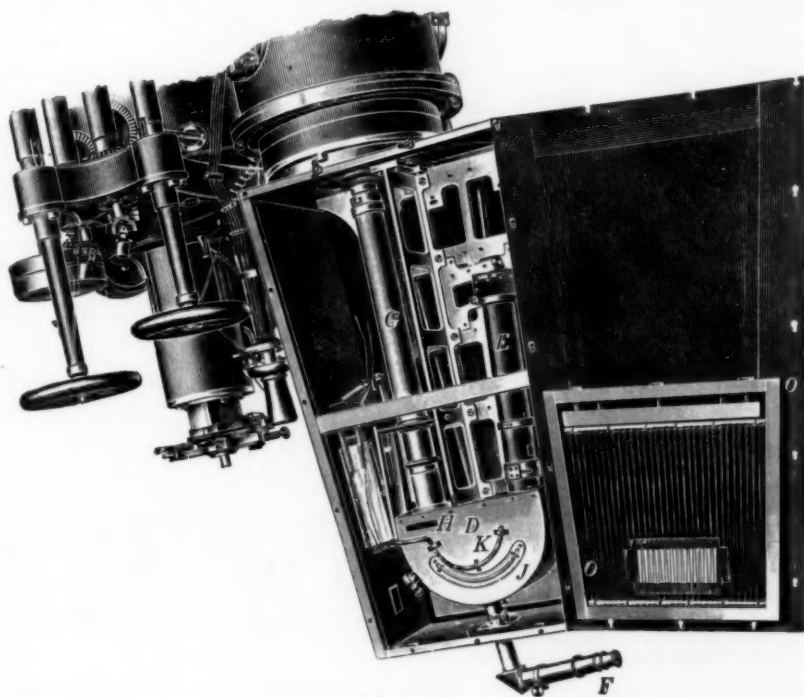

FIG. 2.

the wall of the box; and on the side toward the spectroscope, also at a distance of 1 cm from the frame, a cover of bright sheet metal is fastened, inclosing the whole heater. (In Fig. 2 the cover is removed to make the heating coil visible.) There is, therefore, a free circulation of the air about the heaters, which is an important matter, as otherwise there may easily be marked heat strata, and consequently unequal heating of the different parts of the apparatus. In order to produce a circulation of air in the box by the heater itself, I limited the extent of the heater

to that part of the box which lies at the lowest point during the observations, as is seen in the figure, instead of covering the whole surface of the box by the heater. On turning on the current the warm air will first rise and displace the cooled air at that point. The thermometers do not rise and break off the current until the warmed air has risen to them (at K and similarly below the prism-box). In order to still further exclude injurious air stratification in the box, I separated the heating into two wholly independent parts. The upper thermometer K regulates only the current in the ·heater on the cover, while the thermometer beneath the prism-box independently regulates the lower heater. If both thermometers were therefore set for the same temperature before the beginning of the observations, it is certain that just this temperature will be continuously maintained on both sides of the prism-box.

The ends of all the conducting wires—two from each heater and two from each thermometer, or eight in all—are connected together in binding posts outside of the box. The eight wires leading to these posts are united in two cables of four strands each, one for the upper and one for the lower heater. The heating wires are distinguished by their color from those leading to the control thermometers. The binding posts for the thermometers contain apertures too small for holding the wires of the heating currents, so that confusion in attaching the wires to the binding posts cannot occur.

The cables are five meters long, and can follow all the motions of the refractor; they lead to a switch board attached to the observing chair, the arrangement of which is shown in Fig. 3.

This switch board also has two precisely similar, separate halves, the right corresponding to the upper and the left to the lower heating circuit. The ends of the cable wires are inserted at the plug contacts $O_1 O_2 O_3 O_4$ and $V_1 V_2 V_3 V_4$, the larger size of the plugs for the heating current preventing mistakes. The current from a single Leclanché cell is led in by the posts A. It passes through the electromagnets of the two relays E_1 and E_2, set at zero current, and to the posts $O_1 O_2$ and $V_1 V_2$, which

are connected by the cables with the control thermometers. If the mercury columns of the thermometers are in contact with the platinum wires, the relay current is closed, the armatures of the magnets are attracted and the heating current is broken off. If one of the thermometers falls below the temperature for which it is set, the proper relay current is broken and the released armature closes the corresponding heating current. The relay current is made as small as possible, only 0.011 amperes, in order
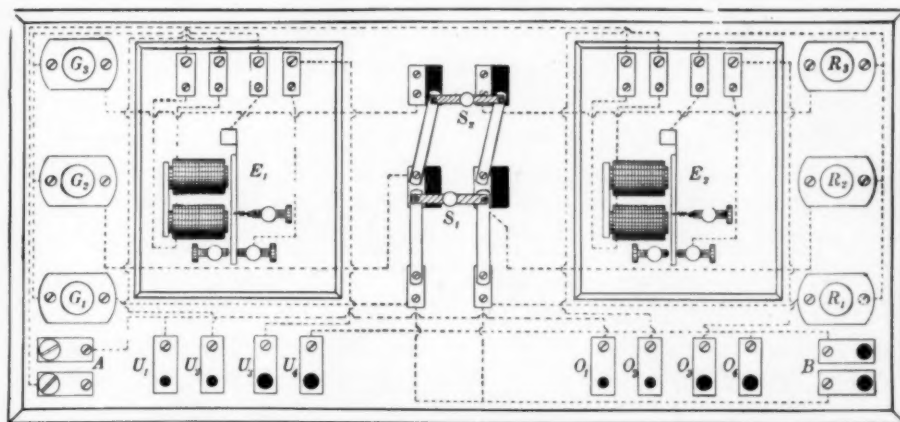

Fig. 3.

to diminish as far as possible the sparking and consequent oxidization at the mercury surface. The heating current is taken from the 110 volt observatory mains and led to the switch board by the two binding posts B. It is first conveyed to the two double switches S_1 and S_2 which throw in three different resistances as needed, changing the current strength. I use for resistances ordinary incandescent lamps, which are screwed into the sockets $R_1R_2R_3$ and $G_1G_2G_3$. In socket R_1G_1 and R_2G_2 I employ lamps of 110 volts and 16 c. p.; in R_3 and G_3 lamps of 65 volts and 10 c. p. When both switches are turned to the right (as S_2 in Fig. 3), the current only goes through the lamp R_1 or G_1, from thence to the relays, and if these are closed, through the cable to the heater, being of strength 0.47 ampere. If the cir-

cuit is closed at S_1 by turning the switch to the left, as shown in the figure, the current divides between the lamps in parallel R_1 and R_2 or G_1 and G_2, rising to 0.62 amperes. On throwing in the third pair of lamps R_3 and G_3, we finally get a current of 0.72 amperes.

The use of the lamps for resistances furnishes a convenient check on the correct working of the heater. In order to further reduce the brightness of the lamps, which are already rather weak with the small current, and at the same time to readily distinguish between the two heaters, I colored the lamps of the upper heater red, and of the lower heater green. The observer can always tell whether everything is working properly, without looking at the switch board, by noting the successive lighting up of the red and green lights at uniform intervals.

I arranged the dimensions of the contact thermometers so that $1°$ C. has a length of 3 mm. When the heaters are in action the variations of the thermometers do not quite reach $0°.1$. An almost absolutely constant temperature must prevail in the interior of the prism-box, as these slight variations of short period are considerably diminished by the metal wall and the inner mass of air. The readings of the thermometer J in fact almost never show a change of over $0°.1$. The following series of observations serves to illustrate the satisfactory operation of the apparatus, which has been in use for a year past. The evening of November 8, 1900, was clear, and I began observations at 4 : 30 P. M., obtaining the following readings θ of thermometer J, and of a thermometer W indicating the temperature of the air in the dome.

Time	W	θ
4^h 30^m	$+10°.2$	$10°.9$
5 30	9.2	10.9
5 45	9.1	10.9
6 30	8.1	10.8
7 40	7.7	10.8
9 30	7.2	10.8
10 30	6.7	10.8

The heating remained in action over night, and when I again began observing next morning the temperature had not changed at all, being at

$$17^{\text{h}}\ 0^{\text{m}} \qquad +\ 4^{\circ}.5 \qquad 10^{\circ}.8$$
$$18\ \ 0 \qquad\qquad 3.2 \qquad 10.8$$

Only an automatic heating device could have kept the prism temperature absolutely constant for so long a time while the air temperature fell 7°.

In conclusion let me say a few words as to the mode of setting the platinum contacts of the metal thermometers. The intention is to maintain the temperature of the prisms constant from the time of making this setting. For this the contacts should be set to correspond with the instantaneous temperature of the prisms, which is, however, unknown, since the readings of the thermometer J do not accurately record the prism temperature, as has been shown above. I have previously demonstrated[1] that a body obeying the law of cooling always has very nearly the temperature that prevailed in the surrounding air $1/\gamma$ minutes earlier. For spectrograph III $1/\gamma = 92$ minutes, and hence, if no other disturbances have meanwhile occurred, the prisms will be nearly at the temperature the air had one and one-half hours before. A Richards thermograph, attached to the refractor near the spectrograph is used for determining this temperature. After some experience it is possible from the reading of the thermograph, in combination with the thermometer J, to set within $0^{\circ}.1$ of the instantaneous temperature of the prisms.

[1] *Zeitschrift für Instrumentenkunde*, **17**, 16, 1897.

ON THE SPARK DISCHARGE FROM METALLIC POLES IN WATER.[1]

By Sir Norman Lockyer.

During the appearance of the new star in the constellation *Auriga*, which was discovered in January 1892, the Kensington photographs was the first[2] to show that several of the brighter lines were accompanied by absorption lines on their more refrangible sides.

This appearance I explained on the hypothesis that we were dealing with at least two bodies, one giving a radiation, and the other an absorption spectrum, the differential movements of which could be determined by the changes of wave-lengths observed.

In a paper[3] published in the year 1899, Dr. J. Wilsing made the suggestion that, in view of the great velocities shown by the large displacements of the lines in the spectra of new stars, and the occurrence of these displacements in the same direction, some other cause of them was probably at work, and he suggested that the cause might be high pressure, which drives the line towards the red.

THE FIRST OBSERVATIONS OF NON-SYMMETRICAL EMISSION.

The non-symmetrical development of emission lines is of frequent occurrence in ordinary arc spectra. Typical photographs of such phenomena were referred to by me in illustration of papers communicated to the Royal Society more than a quarter of a century ago on peculiarities of emission and absorption spectra.[4]

The following extracts from parts of these communications will serve to indicate the facts observed at that time:

PHOTOGRAPHS SHOWING NON-SYMMETRICAL LINES.

I. Spectrum showing two *Ag* lines at about wave-lengths 4054.3 and 4210.0. Both lines are fluffy and reversed ; the less refrangible line is much more strongly expanded on its more refrangible side, and is carried up to a much greater height as a radiation line than its other side. The more refrangible line is more symmetrical, but presents the same phenomenon to some extent, only in the opposite direction, its less refrangible side being the most developed.

II. Spectrum of *Rb*, showing line at wave-length 4202. Here the two ends of the line are produced by radiation alone, the central portion showing absorption on its more refrangible side, with fluffy shading on its less refrangible side.

Afterwards, when higher dispersions became available, the investigations of Messrs. Humphreys and Mohler on the effect of pressure on spectrum lines[1] showed that the actual wave-length of a line was increased by pressure ; thus Humphreys[2] states "the wave-lengths of all fine and sharp lines, *and also of the reversals of heavy ones*, increase with increase of pressure around the arc, no matter how the lines may spread out, symmetrically or chiefly towards either side."

In the case of pressures of twelve atmospheres, a shift of scarcely 0.05 tenth-meter was observed by Messrs. Humphreys and Mohler. Eder and Valenta,[3] in their work on the spark spectra of argon and sulphur under pressure, obtained a displacement amounting to as much as one tenth-meter. With flame spectra of the easily volatile metallic salts, small displacements, averaging 0.4 tenth-meter, were observed by Ebert,[4] and were explained by him as being due to an unsymmetrical broadening of the lines towards the red.

Dr. Wilsing thought that such investigations suggested[5] "the direction which must be taken in the experiments for pro-

[1] ASTROPHYSICAL JOURNAL, **3**, 114–135, 1896; **4**, 175–181, 249–262, 1896; **6**, 169–232, 1897.

[2] ASTROPHYSICAL JOURNAL, **6**, 183, 1897.

[3] *Denkschriften der K. Akad. der Wiss. zu Wien.*, **64**, 1–39 ; **67**, 97–151.

[4] *Wied. Ann.*, **34**, 34–90, 1888.

[5] ASTROPHYSICAL JOURNAL, **10**, 115, 1899.

ducing shifts of lines without motion in the sight-line, and ultimately for producing double spectra."

Wishing to avoid the experimental difficulties necessarily connected with the employment of high pressures, he made use of the fact that very high tensions are produced when electric *sparks* are discharged in liquids.

He employed a large induction coil, with a spark gap inserted in the secondary circuit, in connection with a battery. With the passage of each spark "a blinding discharge took place between the electrodes in the water, giving a very intense continuous spectrum crossed by faint lines." The discharge spectra in water and air were photographed on the same plate with a spectrograph, the scale of the spectrum being about 50 mm between λ 4800 and λ 4600, and the accuracy of the determination of the wave-lengths of the sharp lines could be obtained within a few hundredths of a tenth-meter. Further, several plates were employed which were secured with a grating spectrograph of high dispersion, and with a large prism spectrograph.

Dr. Wilsing investigated in this way the spectra of the metals iron, nickel, platinum, copper, tin, zinc, cadmium, lead and silver, and arrived at the conclusion that "now there occur displacements of lines and double lines which are in every respect similar to those in the spectra of *Nova Aurigæ*." Pressure, then, according to Dr. Wilsing, is the cause of the duplication and broadening of the lines in the spectra of new stars.

The great importance of this result for stellar spectroscopy rendered it imperative to repeat the experiments, and I at once commenced them, using the large Spottiswoode coil, capable of giving a 42-inch spark in air, controlled by placing a large glass plate-condenser in the secondary circuit, so that a spark of length 3 mm was obtained in air, and about 0.5 mm in water. The photographs of the more intense lines in the water-spark spectrum showed very distinct reversals.

The work was postponed a little later owing to this coil being no longer available, but it was again resumed with a smaller (10-inch) coil while waiting for a new large one which is under construction.

With this coil the investigation has been extended by photographing the spark spectra of several other metals in water, and these have furnished material for a more general classification of the attendant phenomena.

The coil used for producing the discharge, being capable of giving only a 10-inch spark, had a 1-gallon Leyden jar placed in parallel with the secondary circuit. The spectrograph employed was a large concave Rowland grating of 6 inches (152.4 mm) diameter, ruled with 14,438 lines to the inch (568.44 per mm), and having a radius of curvature of 21 feet 6 inches (655 cm). The first order spectrum was employed, arranged to photograph the region of the spectrum from λ 3800 to λ 4800, occupying a length of 18 inches (457 mm) on the plate. Distilled water was used in all cases.

Of the metals so far examined (iron, silver, lead, copper, zinc, and magnesium), only three — iron, zinc, and magnesium —show reversals of the principal lines, and those of zinc are very weak.

In all cases the lines of the spectrum of the spark in water are much broader than the corresponding lines in the spectrum of the air-spark. From an examination of the different photographs, however, showing many lines of varying degrees of intensity, it appears that the broadening is, for the most part, of a similar nature to that observed in the arc spectrum in air when an excess of material is introduced between the poles.

THE PHENOMENA PRESENTED BY THE SPARK IN WATER.

A. GENERAL.

In the cases of iron and magnesium, many lines undergo complete reversal, for example, the following :

Iron		Magnesium
λ 4045.98	λ 4325.94	λ 3829.50
4063.76	4383.72	3832.45
4071.91	4404.93	3838.44
4271.93	4415.29	
4308.06		

As shown by the enlargements, this reversal is not always symmetrical with the original bright line, and the part of the emission line on the red side of the reversal is the brighter. It will be evident that in such cases if the exposure is insufficient for the less intense component to be photographed, the appearance of a bright line in a position greatly displaced towards the red will be presented, as is shown in the line of iron at λ 4260.64.

In the case of copper, we have stopped apparently at such an intermediate stage, and the phenomena observed thus appear to agree more closely with those described by Dr. Wilsing. In this case no reversals have actually taken place, and the only lines seen in the water-spark spectrum present the appearance of broad bands, considerably displaced towards the red, and having their more refrangible edges rather sharply defined by absorption, which is not otherwise manifested, while the less refrangible edges are very diffuse.

With zinc two of the lines in the strong group of three in the blue-green region show reversal, the absorption line being nearly normal, separating parts of the emission line of very different intensities. These lines, λ 4722.34 and λ 4810.72, are much more intense on the red side of the central absorption line. In the remaining line of the triplet at λ 4680.32 there is no reversal, but the maximum of intensity of the emission line is also shifted towards the red.

B. CLASSIFICATION OF THE DIFFERENT PHENOMENA PRESENTED.

Considering the photographs obtained with various exposures and conditions, the phenomena observed may be grouped as follows: (1) Broadened bright line. (2) Broadened bright line with central absorption line. (3) Broadened bright line with non-symmetrical absorption (maximum of emission towards red).

1. *Broadened bright line.*—This appearance is well shown in the spectrum of copper and the under-exposed spectrum of iron.
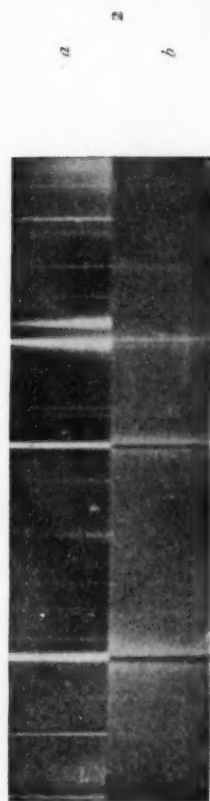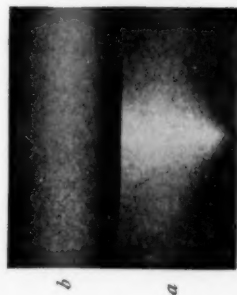
The broadened line is not of uniform intensity throughout its

PLATE XIII.

Mg 3829, 3832, 3838

Mg 4481

SPARK DISCHARGE IN WATER (LOCKYER.)

4383   4404   4415

4045·98
4065·76
4071·91

427I·93
4282·57
4308·08
4315·26
4325·94

4383·72
4404·92
4415·29

a
b
1

a
b
2

breadth, being stronger on the blue side, which is terminated almost abruptly, while the border towards the red is more diffuse.

2. *Broadened bright lines with central absorption.*—This is well shown in the central line of the violet triplet of iron at λ 4063.76.

3. *Broadened bright line with non-symmetrical absorption* (*maximum of emission towards red*).—The best examples obtained of this type of reversal are in the spectra of iron. The

Type	Example	Remarks
1. Complete reversal (strong)....	4415.29 (*Fe*)	Both components of bright line shown strongly, red side most prominent.
2. Complete reversal (weak).....	4415.29 (*Fe*) (another photograph)	Both components of bright line shown, red side much the stronger.
3. Partial reversal (weak)........	4282.57 (*Fe*)	Appearance of a bright line with a dark border on more refrangible side.
4. Partial reversal (weaker)......	4282.57 (*Fe*) (another photograph)	Bright component predominant, dark line only just visible.
5. No reversal.................	4315.26 (*Fe*) and many other weak lines.	....

strong line at λ 4260.64 in the water-spark shows the most decided asymmetry, the less refrangible component of the underlying bright line being seven or eight times stronger than the part on the violet side of the absorption line. There appears to be no suggestion, either of the line being duplex, so that the asymmetry cannot be explained as due to the interaction of two neighboring reversals of varying intensities.

In the case of the absence of any line at λ 4481.30 in the spark in water, it may not be owing to the balance of absorption and radiation, but to a special peculiarity of this line. From

many considerations, I regard λ 4481.30 as a high temperature line only, and therefore it may be that the cooling action of the water envelope surrounding the water spark entirely prevents the production of this radiation.

From these considerations it appears evident that, if proper exposures be given, lines may be photographed in the spectrum of iron, say, which show all the phenomena described by Dr. Wilsing, but so related to each other and the complete stage — that of reversal, symmetrical or unsymmetrical — that it is impossible to regard them as anything abnormal. A typical set of lines illustrating these points, beginning with complete reversal with maximum of emission towards red is as shown in above table.

C. VARIATION OF INTENSITIES.

The most prominent lines in the water-spark are not always the chief lines of the air-spark. This is well shown in the spectra of iron and copper.

Many of the lines in the spark of iron, if their intensities are compared under the two conditions of sparking, show distinct inversions. A typical instance of this occurs with the lines at λ 4422.74 and λ 4427.48. With the spark in air λ 4427.48 is quite twice as strong as λ 4422.74, whereas in the water-spark there is scarcely any trace of a line at λ 4427.48, the 4422 74 line being, however, easily seen.

Another example, slightly less prominent, is found in the lines at λλ 4315.26 and 4337.22. With the spark in air these lines are almost equal in intensity, but in the water-spark λ 4315.26 has about three times the intensity of λ 4337.22.

In the case of copper, in the ordinary spark the most prominent lines are those at λ 4275.32 and λ 4651.31. In the water-spark spectrum the line at λ 4587.19 is almost as strong as either of the lines just mentioned, although in the ordinary spark it is much weaker.

APPLICATION TO STELLAR SPECTRA.

I will next consider the bearing of these results on the explanation of certain features of the spectrum which is charac-

teristic of new stars. It has been seen that in the water-spark the position of the absorption undergoes little if any change of position, while in the case of non-symmetrical reversals, a bright line may be observed greatly displaced towards the red.   In the new stars, on the other hand, the absorption lines are greatly displaced, the accompanying bright lines occupying in compari-son normal positions.   The facts are as follows :

In the case of *Nova Aurigæ* the emission lines had practically normal wave-lengths, but the displacement of the dark line at *Hε* was about 10.7 tenth-meters towards the violet, indicating a velocity of approach of about 500 miles per second.

The recent new star in *Perseus* exhibited the same normal positions of the bright lines, and indications of even greater dis-placements of the dark lines, at one time amounting to 15 tenth-meters at *Hε*, representing a velocity of approach of the body producing the dark-line spectrum of over 700 miles per second.

These values differ enormously from those produced by pressure. The amount of shift produced by subjecting the light-source to pressure is given by Humphreys and Mohler, in the paper above referred to, as follows :

λ	Shift in tenth-meters	Atmospheres
4045.98	0.009	6
4045.98	0.020	11¼
4383.72	0.016	9¾
4383.72	0.026	11½

We find then that the known direct effect of pressure on the radiation or absorption lines is the same, in quality, in water as in air ; that is, we get displacements in the *opposite* direction to that we observe the dark lines to occupy in the spectra of *Novæ*, and we find further that the amount of shift observed in the spectra of new stars differs not only in this respect but also in degree, thus :

Spark in water	New stars
1. Absorption lines least shifted.	Absorption lines most shifted.
2. Radiation lines most shifted.	Radiation lines least shifted.
3. Absorption shift small.	Absorption shift enormous.

It would thus appear that the pairs of bright and dark lines shown in the spectra of new stars do not arise from the cause which produces the appearances presented in the spectrum of the spark in water.

My thanks are due to Mr. C. P. Butler, who obtained and discussed the photographs of the spark spectra, and who, together with Dr. Lockyer, assisted me in the preparation of the paper, and to Mr. F. E. Baxandall, who checked the wave-lengths of the lines discussed and studied the behavior of the lines representative of the different phenomena.

PHOTOGRAPHIC WORK OF THE EXPEDITION FROM THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

TOTAL SOLAR ECLIPSE, MAY 17–18, 1901, SAWAH LOENTO, SUMATRA.

By HARRISON W. SMITH.

AMONG the various expeditions which assembled in Sumatra for the purpose of observing the recent eclipse, the party sent out by the Massachusetts Institute of Technology was unique in that it was prepared to make scientific observations apart from the eclipse and independent of the weather. The valuable set of pendulums of the United States Coast Survey was loaned for the use of the party, and pendulum observations were made at a new station in Sumatra as well as at a station in Singapore, where similar observations had previously been made by other observers. The outfit of the party included also a magnetometer and a dip-circle for detecting variations in the Earth's magnetism during the eclipse. The results of these observations are recorded in the *Technology Quarterly* for March, 1902.

Professor Alfred E. Burton was in charge of the expedition, and was accompanied by Mr. George L. Hosmer, of the Institute, Mr. Gerard H. Matthes, of the United States Geological Survey, and the writer. It is the purpose of this article to describe the photographic work of the expedition. By sailing from New York direct to Genoa, where we met the steamer "Koningin Regentes" of the Netherlands Steamship Co., we were enabled, notwithstanding the distance, to make the journey with very little difficulty. This route was not only the most direct, taking us by way of the Suez Canal and the Red Sea to Padang, the capital of the West Coast, near the central line of the path of the Moon's shadow, but was also the most convenient, for it involved only one transfer of our instruments after leaving New York.

We had, in addition, the pleasure of becoming acquainted, on board the "Koningin Regentes," with the Netherlands party consisting of Professor W. H. Julius, Professor A. A. Nijland, Mr. Wilterdink, and Mr. Hubrecht, and with the English party, Professor H. F. Newall and Mrs. Newall, of Cambridge, Mr. F. W. Dyson, of the Greenwich Observatory, and Mr. Atkinson. After a most enjoyable voyage, we arrived on April 6 at Padang, where we were received with unbounded hospitality by the officers of the Netherlands government and by the United States consular agent. We are under great obligations to these gentlemen for the invaluable assistance which they freely rendered to us during our whole visit, but more particularly for their suggestions in aiding us to select a site for our station. The decision in this important matter had purposely been postponed until our arrival, for, although we had carefully studied the meteorological reports which the Netherlands government had specially prepared for the assistance of astronomers, we wished to have the opportunity of considering the local conditions on the spot. After consultation with the governor and with Mr. Delprat, the director of the government railroads, we soon decided on a location at the end of the railway at Sawah Loento, near the Oembilien coal mines. This point is well beyond the ridge of the coast range of mountains, and thus, to a certain extent, protected from the rains of the coast. At Sawah Loento we had the assistance of Mr. van Lessen, the manager of the mines, and of Mr. Sieburgh, the resident *comptrolleur*, from whom we obtained building materials in abundance and the labor of the convicts at the mines. Our station was on a ridge about a mile and a half from the town; near us was Dr. Mitchell, of the United States Naval Observatory expedition, while about two miles distant Professor Newall had established his camp.

Our photographic outfit included three lenses of 76 mm (3 inches) aperture and 343 cm (135 inches) focus, made by Lundin, of Cambridge, two of which were loaned by Professor E. C. Pickering. We carried also a 3-inch lens of 102 cm (40 inches) focus, which, though not adjusted for photographic work, had

been successfully used in Georgia in photographing the faint elongations of the equatorial streamers. This lens was designed for use both with ordinary plates and with isochromatic plates and a color screen. Of the other three lenses, one was to be used for the corona while two were for photographing regions of the sky east and west of the Sun in the search for intra-mercurial planets. The cameras were long, square boxes of pine sheathing, and were taken apart before leaving Boston and shipped in crates; this method of construction being found very light and especially satisfactory on account of the rigidity of the cameras after being mounted. All four cameras were fastened to one frame-work six feet (183 cm) wide by eight feet (244 cm) high, which hung from a polar axis supported on brick piers eleven feet (335 cm) in height. The fact that our station was only a few miles from the equator, and that the eclipse occurred a short time after noon, made this method of mounting very convenient. The polar axis was a steel shaft 32 mm (1¼ inches) in diameter on which the frame was supported by two brass straps passing over it. Thus the whole cluster of cameras — two on one side of the axis and two on the other — formed one rigid structure so balanced as to swing freely through the necessary angle. Before the cameras were placed in position they were focused on a dark window in a blue house at a distance of about two miles. A base line was measured and the distance accurately determined by triangulation; the focus was then corrected for parallel rays, and finally the cameras were placed in position and the focus tested by obtaining trails from *Arcturus*, whose declination is very nearly the same as was that of the Sun at the time of the eclipse. The trails, however, showed that the focus was practically correct.

The driving of the cameras was accomplished by means of an electric motor acting through a train of gear wheels on a tangent-screw. One end of the screw passed through a nut fixed at the end of the frame at a distance of 259 cm (102 inches) from the center of the polar axis, while the other end was suitably supported on a brick pier. Fig. 1 shows the arrangement of the

motor, gearing, and tangent-screw, with a corner of the frame, before the cameras were placed in position. The motor was maintained at the proper speed by means of an exceedingly ingenious method of clock control devised by Mr. Willard P. Gerrish, of the Harvard College Observatory, after a suggestion
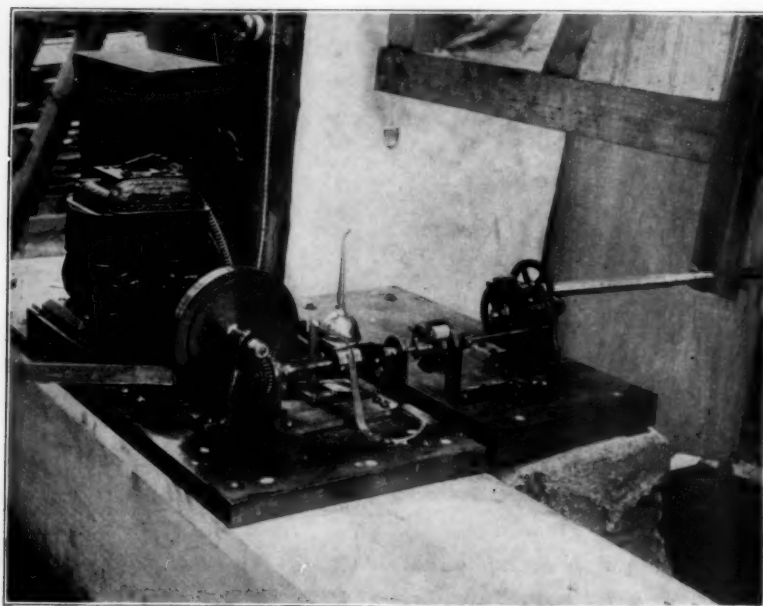
FIG. 1.

by Professor E. C. Pickering, which may be described as follows: A flexible coupling connects the motor to a comparatively heavy fly wheel, which in turn is joined by a worm and gear to a shaft so designed that it will make exactly one revolution per second when the tangent-screw is being driven at the desired speed. This shaft, which may be seen at right angles to the motor shaft, is provided with a make and break contact (about half way between the bearings) so arranged that a brush resting on a disk, composed half of brass and half of vulcanite, closes a circuit during one half of a revolution and opens it during the other half. Fig. 2 is a diagram of the apparatus and the electrical connections, show-

ing at K this contact disk, at M the motor, at R a relay, and at P the pendulum of the controlling clock, which beats half seconds. Attached to the end of the pendulum rod is a strip of platinum which, during each swing of the pendulum to the left of its middle position, cuts through a meniscus of mercury, thus alternately opening and closing the relay circuit at intervals of one half second. In the figure the menis- cus of mercury is shown, for clear- ness, slightly be- low the platinum tip. This relay contact and the contact K are elec- trically connected in series with each other and with the motor, and are then joined to a storage battery of
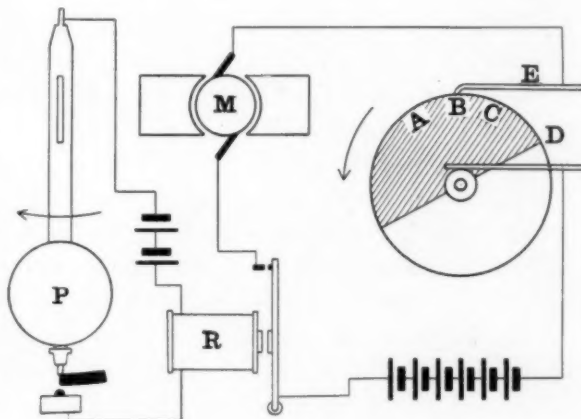
FIG. 2.

a size sufficient to furnish considerably more power than neces- sary to drive the motor at its proper speed. Thus the motor is driven by a series of impulses succeeding each other at intervals of one second, for the motor can only receive current when both the relay and the contact K are closed. The figure represents the condition of things when the pendulum is just on the point of closing the circuit while the brush of the contact disk is on the point $C;$ so that current will flow until the disk, revolving in the direction represented by the arrow, has brought the point D under the brush, thus breaking the circuit. If the acceleration represented by the arc BD is sufficient to maintain the proper average speed, the point B will again be under the the brush when the clock closes the circuit one second later; if, however, this acceleration is insufficient the motor will drop behind a little and when the circuit is closed the point A, for

example, will be under the brush so that the acceleration will continue for a greater interval represented by the arc *AD*. If, on the other hand, the acceleration were too great, the point *C* would have arrived under the brush when the circuit was closed and the acceleration would continue during the interval *CD* only. The device constitutes a form of electrical cut-off in which the clock closes the circuit while the motor opens it after taking the amount of energy required, thus maintaining the average speed absolutely constant. The speed, of course, rises and falls each second, but these fluctuations may be reduced to any desired amount by increasing the weight of the fly wheel and by a slight modification of the electrical connections. Let the motor be connected permanently to the battery, but with a resistance, inserted in series with the armature, of such magnitude as to reduce the speed of the motor somewhat below its normal rate, the control device may then be used to short-circuit this resistance, thus producing a small acceleration each second and gaining the desired regulation with very small fluctuations in speed. A portable storage battery of six five-ampere cells was found sufficient to drive the motor. Through the courtesy of the ship's officers the battery was charged on the steamer just before reaching Padang, and was maintained in perfect condition by means of a bichromate battery. This method of driving the cameras was found most satisfactory by the writer and gave no trouble whatever. The clicks of the relay and the hum of the motor during the short intervals in each second when it is receiving current, produce a rhythmical sound by which one very soon learns to judge accurately whether or not the motor is "in step" with the clock. For this reason, the method possesses a distinct advantage over many of the other forms of driving devices in that, having once completed the adjustment, there is no possibility (aside from an exceedingly improbable change in the rate of the clock) that the apparatus will fail to work correctly without the operator becoming instantly aware of the fact.

Of the three cameras of 343 cm focus, two were used for photographing the sky near the Sun, and were designed to take two

8 × 10 plates (20 × 25 cm), each slightly tilted to improve the focus. One very long exposure was to be made with these cameras, so that it was necessary to cut down the number of exposures for the other camera, which was intended to photograph the corona, as it was not considered advisable to attempt any manipulation on one camera while the others were exposing. Unfortunately the long exposure was of no advantage, since the cloud was so dense on each side of the Sun that scarcely any stars are to be found on the plates. Of the exposures with the third camera, the first, of one-half second for the prominences at second contact, and the last, of twenty seconds, were successful. The other negatives were badly fogged by diffused light from the clouds and are of no value. The two referred to are, however, good, clear negatives. The prominences, although not large, are interesting, and there is evidence of a very violent disturbance in the equatorial region on the easterly limb of the Sun. This disturbance is also plainly visible on the negative of twenty seconds' exposure, and is accompanied by a marked crossing of the filaments radiating out, from a point near the equator, to a distance of eight or nine minutes of arc from the Moon's limb. A particularly striking dark arch appears over a prominence on the easterly limb. The polar streamers extend nearly one-half diameter from the Moon's limb and the equatorial streamers on the west, fully one diameter. This negative was undoubtedly exposed at an opportune moment, when an opening in the clouds was passing over the Sun, for it shows very little indication of fog. Toward the end of totality the sky presented a most striking appearance; the diffused light from the clouds about the Sun seemed almost as brilliant as the corona itself, while near the southern horizon the clouds suggested the last colors of sunset. It was particularly noticeable that, notwithstanding the much greater duration of totality, the darkness was apparently no more intense than it had been in Georgia the year before. At no time was the lamp in the camera-shed a necessity.

The plates were all developed in a very weak Ortol developer

for one and one-half to two hours. Fortunately ice was easily obtainable from Padang, for otherwise it would have been impossible to prolong the development to such an extent; in fact, it would have been difficult to develop at all, since the water in the dark-room was not observed below $80°.5$ F. ($27°$ C.). The Seed double-coated plate was used for the corona, as it was found to have yielded excellent results at the eclipse of 1900. With suitable precaution, no more difficulty was experienced in manipulating these plates than the ordinary plates, and the writer is firmly convinced that the negative contains detail in the inner corona which can be reproduced by careful shading of the negative during printing that would have been lost by over-exposure in an ordinary single-coated plate.

In addition to the work already described, an attempt was made to photograph the shadow bands by a method which the writer believes has not previously been tried. Although the results were not satisfactory, they were such as to make it appear that the method was worth another trial, especially since the bands were not sufficiently distinct to give the method a thorough test. In brief, it consists merely in exposing a sensitive plate directly to the bands themselves rather than attempting, with a camera, to photograph them as they appear on a white screen. The exposing is done by means of a sort of large focal plane shutter. Three 8×10 plates were placed in the bottom of a shallow box, giving an area 10×24 inches. Over this was a light-tight screen of two pieces of rubber gossamer cemented together on the rubber side. The screen is attached at each end to a curtain roller, a narrow slot is cut across it at the proper point, and the operation of exposing consists in rapidly drawing this slot over the sensitive plate by means of rolling the screen up on one roller while it is being drawn off from the other. The boxes, for several were used, were laid flat on the ground, and the exposures were made when the bands appeared, but the diffused light was so great that it was hardly to be expected that the bands would appear, and, in fact, the plates indicate over-exposure. As the exposures averaged

about 1/100 second, it is probable that 1/200 to 1/500 second, or, perhaps 1/1000 second would be sufficient for plates of moderate speed. Plates of different speed were used ; but as they all show over-exposure it would probably be advisable in another attempt to err on the side of too short rather than too long exposure. It would be better also to substitute for the the cloth a metallic screen that could be moved at a more uniform rate, and it was found also that a cloth screen twelve inches or more wide, with a slot across nearly its entire width, did not run as smoothly as could be wished, while a metallic screen, on the other hand, could be made to slide freely and would not be affected by having a slot of any width cut across it.

The apparatus was tested successfully on artificial shadowbands before leaving Boston, and it is largely on this account that mention of it is made here in the hope that it may be again tested at a later eclipse.

NOTE ON A PERSONAL EQUATION IN MEASURING PHOTOGRAPHIC SPECTRA.

By B. Hasselberg.

In *Bulletin* No. 15 of the Lick Observatory Mr. Reese describes a systematic discrepancy between measures of a stellar photograph made in different directions, going to show that there is a definite tendency to set the cross-hair of the microscope a little farther to the right (as seen in the microscope) on the dark lines of the comparison spectrum, than on the bright lines in the spectrum of the star. Having by special experiment ascertained that the discrepancy in question, according as the measures are made in the direction from violet to red or vice versa, is not to be attributed to any curvature of the comparison lines, nor to the relative position of the spectra, Mr. Reese concludes that the cause of the phenomenon is an entirely, or almost entirely, physiological one, the perception of a dark line in a bright field being somewhat different from that of a bright line in a dark field. As I have had similar experiences in the course of my researches upon the arc spectra of the metals I think it may be of some interest to give a succinct account of the results arrived at, more particularly as my perception of the spectral lines seems to be contrary to that described by Mr. Reese.

In measuring the photographs of the arc-spectra of metals I have hitherto always placed the plates on the measuring engine in such a way that the measures with increasing readings proceed from red to violet, the cross-hair in the microscope apparently moving from left to right. In this position of the plate, which may be marked I, the solar spectrum is seen to be placed above that of the metal. Every metallic line within a group limited by two solar lines, in a mutual distance generally not exceeding 20 tenth-meters, is thus referred to these solar lines and its

wave-length deduced simply by linear interpolation. With the
view, however, of further controlling the results obtained, a solar
line in every group is also occasionally measured; the agree-
ment of the wave-length of this with the value given by
Rowland shows the correctness of the wave-lengths deduced for
the metallic lines. Now, this agreement is always very close
indeed, but on examining the small residual differences, they
are invariably found to be to a certain degree systematic, the
wave-lengths obtained by me being in almost every case a little
too small, as compared with the values from Rowland's table.
As a specimen of this peculiar circumstance the following list of
solar lines, measured in connection with my researches on the
arc spectrum of molybdenum may be adduced :

TABLE I.

H.	R.	H.−R.	H.	R.	H.−R.	H.	R.	H.−R.
5816.558	.601	−0.043	4358.657	.670	−0.013	4066.508	.524	−0.016
5763.200	.218	− .018	51.200	.216	− .016	50.820	.830	− .010
01.766	.772	− .006	37.193	.216	− .023	45.977	.975	+ .002
5662.737	.744	− .007	08.075	.081	− .006	30.323	.339	− .016
38.477	.488	− .011	07.904	.907	+ .007	10.298	.327	− .029
5560.430	.434	− .004	4288.319	.310	+ .009	07.409	.429	− .020
5497.723	.735	− .012	67.108	.122	− .014	3996.118	.140	− .022
66.582	.609	− .027	48.381	.384	− .003	68.611	.625	− .014
45.255	.259	− .004	25.605	.619	− .014	52.099	.103	− .004
5353.575	.571	+ .004	22.363	.382	− .019	48.227	.246	− .019
22.234	.227	+ .007	04.138	.132	+ .006	41.897	.878	+ .019
5250.808	.817	− .009	02.197	.198	− .001	33.822	.825	− .003
34.787	.791	− .004	4185.040	.058	− .018	16.868	.879	− .011
4810.710	.724	− .014	63.802	.818	− .016	3892.073	.069	+ .004
4772.998	73.007	− .009	56.962	.970	− .008	73.900	.903	− .003
4691.573	.602	− .029	36.675	.678	− .003	52.710	.714	− .004
36.025	.027	− .002	18.696	.708	− .012	3792.472	.482	− .010
4598.286	.303	− .017	14.605	.606	− .001	86.300	.314	− .014
71.261	.275	− .014	00.878	.901	− .023	66.799	.801	− .002
4449.325	.313	+ .012	4098.327	.335	− .008	44.253	.251	+ .002
25.598	.608	− .010	76.102	.101	+ .001	29.954	.952	+ .002
4395.198	.201	− .003	71.893	.908	− .015	17.523	.539	− .016
74.621	.628	− .007	70.912	.930	− .018			

It is evident from the inspection of this table that the num-
bers of the third column include not only the accidental errors
of observation, but also a systematic difference. the value of
which may be found by taking the mean of all the differences

H. — R.   This mean is H. — R. = — 0.009 tenth-meter, by which amount my solar wave-lengths are systematically too small. On applying this as a systematic correction to the differences above, they may be regarded as representing only the accidental errors of observation, from which the probable error of a solar wave-length as determined on my plates relatively to the system of Rowland comes out $\epsilon = \pm 0.007$ tenth-meter.

Now the existence of such a systematic error or personal equation is by itself hardly surprising; the question, however, as to its origin might well be of such a nature as to elicit differences of opinion.   Among the suppositions that may be put forward in this respect, the most natural seems to be to assume that the setting of the cross-hair of the miscroscope on the spectral lines takes place somewhat differently, according to the direction in which the measures are executed.   With the view to test this hypothesis more closely a series of solar lines, the wave-lengths of which had been determined in the position I of the plate on the occasion of the researches upon the spectrum of tungsten now in progress, was again measured in the opposite position of the plate, or in the direction from violet to red with increasing readings.   This position may be designated as II. The results obtained are embodied in the first two columns of the following table, containing in addition the differences II—I and the mean results, together with their comparison with the corresponding values of Rowland.

It appears from the numbers of the third column that there exists between the determinations in the two positions a difference, certainly very small indeed, but nevertheless clearly pronounced, which indicates a systematic correction of $+0.003$ tenth-meter to be added to the measures in the position I.   On comparing the means in the fourth column with Rowland it is seen, however, that this correction does not suffice to explain the systematic difference between my determinations and those of Rowland, but that there remains a mean discrepancy of -0.006 tenth-meter thus far not accounted for.   Could it be assumed that the measures of Rowland were systematically too great by

this amount, the agreement of our values would be perfect. Besides that it may be remembered that the systematic difference

TABLE II.

I	II	II−I	$\frac{I+II}{2}$	R.	H.−R.
4852.730	.736	+0.006	.733	.743	−0.010
36.050	.059	+ .009	.055	.059	− .004
10.704	.715	+ .011	.710	.724	− .014
4789.838	.839	+ .001	.838	.849	− .011
72.995	73.002	+ .007	.998	.007	− .009
56.697	.702	+ .005	.700	.705	− .005
36.022	.021	− .001	.022	.031	− .009
18.596	.605	+ .009	.600	.601	− .001
4690.309	.310	+ .001	.310	.317	− .007
78.341	.347	+ .006	.344	.347	− .003
61.707	.712	+ .005	.710	.712	− .002
38.180	.189	+ .009	.184	.193	− .009
19.456	.464	+ .008	.460	.468	− .008
4594.291	.295	+ .004	.293	.297	− .004
71.263	.269	+ .006	.266	.275	− .009
58.818	.819	+ .001	.818	.827	− .009
41.693	.681	− .012	.687	.690	− .003
12.904	.908	+ .004	.906	.906	.000
4491.528	.565	+ .037	.546	.570	− .024
56.777	.780	+ .003	.778	.794	− .016
43.358	.356	− .002	.357	.365	− .008
17.858	.868	+ .010	.863	.884	− .021
00.548	.546	− .002	.547	.555	− .008
4379.377	.394	+ .017	.385	.396	− .011
68.054	.067	+ .013	.060	.071	− .011
44.439	.451	+ .012	.445	.451	− .006
27.269	.263	− .006	.266	.274	− .008
13.029	.038	+ .009	.033	.034	− .001
08.078	.080	+ .002	.079	.081	− .002
07.908	.917	+ .009	.912	.907	+ .005
4299.140	.147	+ .007	.143	.149	− .006
74.332	.341	+ .009	.336	.348	− .012
58.475	.490	+ .015	.482	.477	+ .005
39.501	.511	+ .010	.506	.525	− .019
20.493	.506	+ .013	.500	.509	− .009
01.078	.075	− .003	.077	.089	− .012
4182.916	.914	− .002	.913	.922	− .009
63.796	.815	+ .019	.806	.818	− .012
42.017	.025	+ .008	.021	.025	− .004
25.771	.781	+ .010	.776	.776	.000
06.583	.576	− .007	.580	.583	− .003
4087.243	.245	+ .002	.244	.252	− .008
70.409	.419	+ .010	.414	.431	− .017
52.078	.083	+ .005	.080	.070	+ .010
25.962	.972	+ .010	.967	.972	− .005
15.753	.763	+ .010	.758	.760	− .002

between my measures of solar lines as obtained in connection with the spectrum of molybdenum, and the values of Rowland, is the same as in the case of my researches upon the spectrum of tungsten, or H.— R.= —0.009 tenth-meter.

If it be considered that in measuring spectral lines of the *same* appearance, as in the cases above, a systematic error or personal equation nevertheless can exist, it becomes very probable that such a personal equation in a still higher degree may also be met with in case that a metallic spectrum is to be compared with the solar spectrum, the objects to be observed then being of an appearance quite contrary one to the other. The correctness of this view may be inferred from the inspection of the following table, giving the wave-lengths of a group of lines in the arc spectrum of tungsten, as measured in the two positions of the plate.

TABLE III.

I	II	II – I	I	II	II– I
4449.182	.164	— 0.018	4422.010	21.958	— 0.052
45.324	.297	— .027	21.168	.135	— .033
44.650	.592	— .058	20.627	.608	— .019
42.004	41.976	— .028	19.429	.387	— .042
39.904	.845	— .059	18.967	.934	— .033
38.469	.441	— .028	18.609	.584	— .025
37.070	.070	.000	15.891	.845	— .046
35.903	.874	— .029	15.233	.246	+ .013
32.379	.314	— .065	14.042	13.987	–· .055
28.663	.607	— .056	13.173	.142	— .031
27.533	.508	— .025	12.347	.343	— .004
26.087	.052	— .035	11.871	.834	— .037
25.064	.030	— .034			

It is immediately seen that the values in the second column constantly fall short of those in the first, the mean discrepancy being — 0.033 tenth-meter. Thus the systematic error or personal equation in every case numerically amounts to 0.016 tenth-meter to be subtracted in the first position of the plate and added in the second. Now, as in the position I, the measures proceed from red to violet or from left to right in the microscope and the resulting wave-lengths at the same time are too great, it follows that the settings on the metallic lines must have been

made a little to the left of the true position. On the second position of the plate the left and right correspond to violet and red, and as in this case the resulting wave-lengths come out too small, it appears that even now the settings are systematically erroneous in the same direction.

As far as I am concerned, then, the personal equation in the present case is with regard to the direction quite the contrary of that observed by Mr. Reese, but this circumstance need not be regarded as surprising, the whole phenomenon without doubt depending only on physiological peculiarities. As to the amount of the error it may on the other hand be remarked as a curious incident that if the above measurements had been intended for determination of radial velocity, the corresponding correction would in conformity with the result obtained by Mr. Reese have been almost exactly 1 kilometer.

ACADEMY OF SCIENCE, STOCKHOLM,
February 1902.

SOME RESULTS WITH THE BRUCE SPECTROGRAPH.

By WALTER S. ADAMS.

THE VARIABLE VELOCITY OF o *PERSEI* IN THE LINE OF SIGHT.

FIVE spectrograms of the fourth magnitude star o *Persei* $(a = 3^h 38^m; \ \delta = + 31° 58')$ recently obtained by the writer show a large variation in its radial velocity. The results of the measurements are as follows:

1902	February 19						+ 134 km
	February 21						− 77
	March 4						+128
	April 2						−117
	April 3						− 4

The plate of April 2 is under-exposed, and the value given by it is consequently somewhat less accurate than the others in the list. The spectrum of this star is of the Orion type, but the intensities of the helium lines are much less than in the representative stars of the group, while the *Mg* line λ 4481 is hardly visible.

The ranges of variation in this star and in the star η *Orionis*, whose variability was announced in December, are the largest which have hitherto been found among spectroscopic binaries having one component dark.

THE VARIABLE VELOCITY OF δ *LIBRAE* IN THE LINE OF SIGHT.

The well-known *Algol* variable δ *Librae* $(a = 14^h \ 56^m; \ \delta = − 8° 8')$ exhibits a considerable variation in its radial velocity. The following spectrograms have been secured:

1902	March 4					+ 36 km
	March 12					− 123
	April 2					− 97
	April 3					+ 38

The spectrum of this star has been assigned by Miss Maury to Group VIIa of the Harvard classification, but the metallic lines are exceedingly broad and faint, with the exception of the

Mg line λ 4481, which though broad is of moderate strength. The measures are consequently uncertain by several kilometers.

THE RADIAL VELOCITY OF *SIRIUS*.

A considerable number of plates of *Sirius* have been obtained by the writer during the winter in the course of investigations on the photographic efficiency of the spectrograph, and the density of the negative best suited to bringing out the fine lines of the class of spectra to which the star belongs. I have measured ten of these with a view to securing as accurate a value as possible of its radial velocity, and the results are given in the table below. Both of the cameras in use with the instrument have been employed, the series letter *A* indicating the Zeiss anastigmat of 449 mm focus, and *B* the Hastings triplet of 607 mm focus. Numerous checks in the way of plates of *Venus* and the Moon have been applied.

Series No.	Date		No. of Lines	Velocity
B 258	1901	December 18	12	−7.2 km
B 263		December 31	13	7.1
A 301	1902	January 4	16	7.0
A 302		January 4	16	6.6
B 271		January 9	14	6.8
B 278		January 16	17	6.8
B 285		January 24	18	6.9
A 311		February 10	11	6.9
A 329		March 3	13	6.9
B 291		March 12	15	6.5
Epoch	1902.06			−6.87 km

This result, in combination with the value of −15.6 km for the epoch of 1890.09 obtained by Vogel and Scheiner at Potsdam, renders possible a computation of the parallax of the system of *Sirius* through the change in the radial velocity of the principal star during the interval. The method has been treated by several writers, among others Rambaut and Monck.[1] Using the most recent orbital elements by Zwiers,[2] and the results found by

[1] *Sidereal Messenger*, **9**, 289, 1890.

[2] *Kon. Akademie Wetenschappen Te Amsterdam*, Proceedings of meeting, May 27, 1899.

Auwers from meridian observations for the motion of the center of gravity of the system, we obtain for the parallax

$$\pi'' = \frac{1.84}{-6.9+15.6} = 0.''21 .$$

The latest value of the parallax of *Sirius*, as found by Gill from heliometer measures, is 0.''37. The difference is no doubt partially due to the error of the earlier spectroscopic measures, but unless we assume this error to amount to 4 km, the above result would lead us to infer that the true parallax of *Sirius* lies somewhat below the heliometer determination.

THE WAVE-LENGTH OF THE *Mg* LINE λ 4481.

On account of the prominence of the line λ 4481 in the spectra of stars of the Orion and Sirian types an accurate determination of its wave-length is of considerable importance for radial velocity work in this region of the spectrum.   So far as the writer has been able to learn, however, no such determination exists, owing no doubt to the difficulty of securing accurate measurements upon the broad and diffuse line given by the magnesium spark in the laboratory. Accordingly it seemed best to determine its value directly from suitable stars, and for this purpose *Sirius*, γ *Geminorum*, and θ *Leonis* were selected, in all of which the line is sharp, narrow, and of great brilliancy. Ten plates of *Sirius*, three of γ *Geminorum*, and two of θ *Leonis* gave the following results, after elimination of radial velocity:

Sirius

B 258	4481.407
B 263	.386
A 301	.382
A 302	.413
B 271	.407
B 278	.405
B 285	.384
A 311	.389
A 329	.410
B 291	.397

γ Geminorum

B 236	-	-	-	-	-	4481.388
B 243		-	-	-	-	.408
B 249	-	-	-	-	-	.414

θ Leonis

B 272	-	-	-	-	-	.413
A 308	-	-	-	-	-	.396

4481.400

The value 4481.400 has been employed in the determination of the radial velocities of *o Persei* and δ *Librae* given in the present paper.

YERKES OBSERVATORY,
  April 9, 1902.

SOME OBSERVATIONS ON THE RESOLVING POWER OF THE MICHELSON ECHELON SPECTROSCOPE.[1]

By P. Zeeman.

1. On a recent occasion[2] I gave a few observations on this subject. The acquisition of some new data induces me to return to it in this place.

In his "Investigations in Optics," Lord Rayleigh[3] expressed the wish that spectroscopists in possession of powerful instruments would compare the actual resolving power with that of which they are theoretically capable, and remarked that a carefully arranged succession of tests of gradually increasing difficulty would be of especial value.

I remembered these remarks as I tested the very original echelon invented by Michelson.

The echelon at my disposal, made by Hilger, London, consists of thirty plates each about 7.8 mm thick, made of light flint-glass, set with 1 mm steps. A clear aperture of 1 mm is left beyond the width of the largest glass plate. The number of apertures n, operative in the formation of the spectrum, is hereby one more than the number of plates. The mounting was somewhat improvised. Telescope and collimator belonging to a Kirchhoff spectroscope were employed. The telescope had object-glasses of 50 cm focus and 38 mm aperture. It is evident that when the mounting is made especially it is advisable to have glasses of shorter focus, so as to get greater intensity.

Denoting by $d\lambda_1$ the difference of wave-length of spectral lines when they are just distinguishable as separate in the spec-

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[1] Communicated by the author as advance proof of a paper to appear in the Proceedings of the *Amsterdam Academy of Sciences.*

[2] Bosscha, *Collection of Memoirs, Archiv. N'eerl.,* Sér. II, **6**, 319, 1901.

[3] *Phil. Mag.,* 1879, 1880.

troscope, by t the thickness of the plates of glass, and by n the above mentioned number, then we have

$$q_t = \frac{d\lambda_t}{\lambda} = \frac{\lambda}{knt} \; , \tag{1}$$

if

$$k = (\mu - 1) - \lambda \frac{d\mu}{d\lambda} \; .$$

The resolving power is given by

$$r = \frac{\lambda}{d\lambda_t} = \frac{knt}{\lambda} \; . \tag{2}$$

For the green line $\lambda = 5460$ we obtain in the case of our echelon,

$$r = \frac{0.63 \times 31 \times 7.8}{5460 \times 10^{-7}} = 280000 \text{ and } q_t = \frac{d\lambda_t}{\lambda} = 3.6 \times 10^{-6} \; .$$

In the calculation of k I use the following values of the refractive indices given to me by Hilger:

$$\mu_C = 1.5713$$
$$\mu_D = 1.5753$$
$$\mu_F = 1.5853$$
$$\mu_{G'} = 1.5936$$

Henceforth I will denote by q_t the theoretical value of the limit of resolution calculated according to (1), by q_e the experimental value. By means of a Hoffmann direct vision spectroscope the light of the vacuum tubes (driven by a Ruhmkorff) undergoes the necessary preliminary analysis. In some cases absorbing media were therefore sufficient. In some experiments the mercury arc-lamp of Fabry and Perot was used.

2. The very intense *green* line ($\lambda\,5460$) of *mercury* was investigated first. Using the echelon in a position in which two strong lines of equal intensity corresponding to successive orders of the radiation were visible, I could distinguish also five faint, very narrow lines between the principal ones. The distance between two pairs of these lines was very small.

As I could not find a table of the wave-lengths of these feeble radiations, I addressed myself to Messrs. Fabry and Perot. I am very much obliged to Messrs. Perot and Fabry for their kindness

in investigating for me anew the green radiation of the mercury arc *in vacuo*.

The following scheme represents the constitution of this very complex radiation according to their observations. The ordinates are *approximately* proportional to the intensities.

$$\frac{\Delta\lambda}{\lambda}\,10^6 \quad -41 \qquad\qquad -14\,-9.5\;\;0+1.5\;+15\;\;+25$$
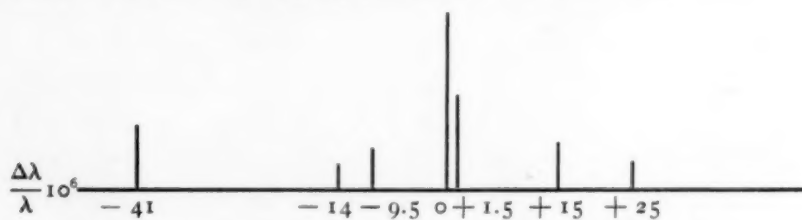
The numbers given are only approximate, especially (-14) and (-9.5).

The radiation $(+1.5)$ was observed by Fabry and Perot only in the radiation of a Michelson tube; it is too close to the principal radiation to be seen separately in the arc light. In the photographic reproduction in the ASTROPHYSICAL JOURNAL [1] of the interference fringes of the green mercury line, the radiation (-41) coincides with the radiation $(+15)$ and is therefore invisible.

I could distinguish very clearly the radiations (-9.5) and (-14) as separate lines. For these radiations $q = \dfrac{d\lambda}{\lambda} = 4.5 \times 10^{-6}$ or $r = 222,000$ and hence q_e rather smaller; calculation gave $q_t = 3.6 \times 10^{-6}$.

Using the *green* line of *thallium* [2] I very easily distinguished the faint radiation at a distance $\dfrac{d\lambda}{\lambda} = 21 \times 10^{-6}$ from the principal radiation, but I could not see as a separate line the the one determined by $\dfrac{d\lambda}{\lambda} = 3 \times 10^{-6}$.

Hence q_e exceeds 3×10^{-6} but is smaller than 21×10^{-6}.

[1] FABRY and PEROT, ASTROPHYSICAL JOURNAL, **13**, 272, 1901.

[2] FABRY et PEROT, *Ann. de Chim. et de Phys.* (7) **16**, 134, 1899.

Indeed for the thallium radiation ($\lambda\,5440$)

$$q_t = \frac{5440 \times 10^{-7}}{0.63 \times 31 \times 7.8} = 3.6 \times 10^{-6}.$$

For the *green* ($\lambda\,5086$) line of *cadmium* it was just possible to see that this line is a double one. The distance of the components is according to Fabry and Perot[1] $\frac{d\lambda}{\lambda} = 5 \times 10^{-6}$. For $\lambda = 5086$ I calculate $q_t = 3.2 \times 10^{-6}$. Hence with the above mentioned echelon it is possible to approach rather nearly the limit of the theoretical resolving power.

3. Perhaps the best series of tests of gradually increasing difficulty can be obtained by observation of the change of spectral lines in magnetic fields of gradually increasing intensities, a Nicol between source and apparatus being used in order to reduce the complexity of the radiation. In this manner all values between, *e.g.*, 0.001 tenth-meter to about 1 tenth-meter can be obtained. Corresponding herewith are the values $q_t = 0.2 \times 10^{-6}$ and $r = 5,000,000$; resp. $q_t = 200 \times 10^{-6}$ and $r = 5000$. The performances of echelons and interferometers and of ordinary spectroscopes with a few glass prisms lie between the limits indicated. This test I have not yet applied systematically to the above mentioned echelon.

In order, however, to show its fitness I will use some observations of Lord Blythswood and Dr. Marchant.[2] In their § 6, "Results Obtained of the Zeeman Effect on the Chief Lines of the Mercury Spectrum," p. 397, these authors communicate observations with an echelon spectroscope concerning the difference in wave-length between the components of the outer components of the sextet of the blue line ($\lambda\,4358$) of mercury. The following table is an extract:

H	$\delta\lambda_3$
5.000	..... tenth-meters
12.100	.....
12.900	0.052
20.000	0.098 ?
21.300	0.09
23.400	0.098

[1] *Loc. cit.* p. 137. [2] *Phil. Mag.* **49**, 384, 1900.

For a value of the field between 12.100 and 12.900 the splitting up of the lines becomes sufficient to make them appear as separate lines *on a photograph* (upon which the measurements were taken). Two lines can, of course, be *seen* separated at a rather smaller distance.

Thus now $q = \dfrac{0.052}{4358} = 11.9 \times 10^{-6}$ and q_e is rather smaller. For the echelons of these observers we have $t = 7.5$, $n = 15$.

With these data I calculate $q_t = 5.3 \times 10^{-6}$.

Thus it appears from the data given in this paper that it is possible to manufacture echelons performing nearly as well as they are theoretically expected to.

THE MECHANICAL EQUIVALENT OF THE UNIT OF LIGHT.[1]

By Knut Ångström.

A FIRST attempt at determining the mechanical equivalent of a source of light had been made by Julius Thomsen[2] as early as 1865, and in 1889 a more precise determination of our present unit was carried out by O. Tumlirz.[3] The total radiation (Q) was measured with a variety of air thermometer, the ratio of the luminous portion to the whole radiation, or the luminous effect of the radiation ($\frac{L}{Q}$) was determined in the manner earlier used by Melloni and Julius Thomsen, by causing the infra-red rays to be absorbed by a layer of water, and thus separated from the luminous rays.

In my former determination[4] of the total radiation of the Hefner amyl-acetate lamp, I became convinced that it was considerably greater than given by Tumlirz.

The method of measuring the luminous efficiency by water absorption is readily seen to be incorrect in theory, and must lead to too high values of the luminous radiation.[5]

Since an accurate knowledge of our unit of light is of great importance in connection with many problems of physics, I decided to resume the question. The treatment of the Hefner lamp is as heretofore in two parts: (1) the determination of the whole radiation; (2) the determination of the ratio of the luminous and total radiation.

The normal lamp with Hefner flame used was obtained from

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[1] From advance proofs from the author of an article also to appear in the *Physikalische Zeitschrift*.

[2] *Pogg. Ann.*, **125**, 348, 1865.

[3] *Wied. Ann.*, **38**, 640, 1889.   [4] *Wied. Ann.*, **67**, 647, 1899.

[5] Tumlirz found the total radiation of a candle at one meter to be 0.0000148 gram-calories per second, and the luminosity to be 2.4 per cent. of this.

Siemens and Halske. For determining the total radiation I employed the compensation-pyrheliometer[1] of my design; this was an excellent instrument, with strips of manganin; for the details of its construction I would refer to the paper above cited. The equality of the temperature of the strips was obtained with the aid of a highly sensitive reflecting galvanometer, and the strength of the heating current with a milliampere meter from Siemens and Halske. The total radiation was measured at two different distances, 50 and 100 cm from the lamp, and the results were in complete agreement. As mean of several determinations this result was obtained:

Total radiation (Q) at a distance of one meter $= 0.00129$ gram-calories per minute, or 0.0000215 gram-calories per second. The error of this determination probably does not exceed 3 per cent.
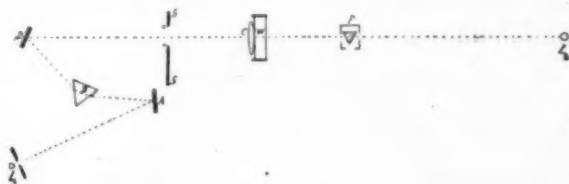
The luminous effect of the radiation was also determined by a new method. That employed by Langley, determining the energy spectrum by the bolometer and obtaining the luminous effect by integrating the resulting curves, is doubtless correct in principle, but is beset with considerable difficulties, especially with weak sources of light. Moreover the prisms and lenses or mirrors of the spectroscope exert a selective effect on the radiation, which is especially conspicuous in the extreme infra-red, and which affects the result in a manner difficult of computation. The following method is free from this source of error:

The radiation of the source under investigation is dispersed by a spectroscope. The invisible portions of the spectrum are cut off by screens, while the luminous rays, on the other hand, are united by a cylindric lens to form a white image on the head of a photometer. A second similar source of light is so arranged as to throw a photometrically equal amount of light directly upon the photometer screen. We are thus dealing with two radiations, physiologically precisely equal in intensity and composition, the first containing luminous rays only, while the second is the corresponding total radiation. If a bolometer or

[1] *Wied. Ann.*, **67**, 633, 1899.

a thermopile is substituted for the photometer screen the energy of the two radiations, and hence their ratio, can be determined.

The arrangement of the experiment will be rendered clear by the figure. L_1 and L_2 are the sources of light ; *ABD* is a mirror spectroscope ; *S* a screen ; *C* a cylindric lens ; *W* a water tank to prevent the radiation from warming the screen; *P* the photometer screen, which can be exchanged for a sensitive thermopile. All these parts are attached to a large optical bench two meters long, and hence can be easily adjusted. The screen *S* can be moved by a micrometer screw perpendicularly to the length of the bench, and was set so as to cut off the spectrum beyond λ 0.76 μ.

In the investigation of sources whose luminous radiation is produced by the incandescence of carbon particles, the source L_1 may be replaced by an incandescent lamp and the current regulated until the color of the light is the same as that of the source to be investigated. This is especially advantageous in the cases where the luminous radiation is weak, and was accordingly employed by me in the investigation of the Hefner light.

I obtained as the mean of many determinations of the luminous effect of the Hefner lamp,

$$\frac{L}{Q} = 0.90 \ \% \ (+ \ 0.04 \ \%).$$

From these determinations of Q and $\frac{L}{Q}$ may now be computed the energy corresponding to our unit of light (energy of luminous radiation on 1 sq. cm at 1 cm distance), and unit of luminosity (energy of luminous radiation on 1 sq. cm at 1 meter's distance). We obtain : 1 unit of light $= 0.009 \times 0.0000215 \times 100^2 = 1.94 \times 10^{-3}$ gram-calories per second $= 8.1 \times 10^4$ ergs per second. One meter candle $= 0.009 \times 0.0000215 = 1.94$

$\times$ 10⁻⁷ gram-calories per second $=$ 8.1 ergs per second.   Hence the mechanical equivalent of the unit of luminous intensity is, in round numbers, 8 ergs per second.[1]

I have thus far only had an opportunity to investigate the luminosity of the radiation of the Hefner lamp and the acetylene flame.   For the latter I obtained as the mean of five determinations,

$$\frac{L}{Q} = 5.5 \ \% \ .$$

This has been previously investigated by Stewart and Hoxie.[2] They obtained by an improved absorption method $\frac{L}{Q} = 10.5$ per cent., a result clearly too large on account of the sources of error of the method.   But a later determination by Stewart,[3] using the method of integrating the energy curve obtained spectrometrically, confirmed the former high value. This is, however, to be explained by an error he makes in the treatment of his results.[4]

On avoiding this error we obtain from Stewart's observations a value of the luminosity between 5 and 6 per cent., in complete agreement with that found above.

It has been long known that the luminosity of the radiation of our ordinary sources of light is very low; but it would seem that the earlier determinations — with the possible exception of those of Langley — were from two to three times too high.

[1] See the statement in DRUDE'S *Lehrbuch der Optik*, p. 445, 1900, where the computations are based on Tumlirz's values.

[2] E. L. NICHOLS, *Physical Review*, 11, 219, 1900.

[3] *Physical Review*, 14, 257, 1901.

[4] He introduces the measures made in a prismatic spectrum into a normal spectrum, and divides each observation by the square of the slit-width expressed in wavelengths.   In this way he really twice converts his prismatic spectrum into a normal one.

UPSALA,
January 1902.

SELECTIVE ABSORPTION AS A FUNCTION OF WAVE-LENGTH.

By George E. Hale.

Readers of my note on the spark spectrum of iron in liquids, which was published in the last number of this Journal, can hardly have failed to notice that the first reversals of the iron lines appeared at the more refrangible end of the spectrum, and that as the conditions became increasingly favorable to elective absorption, bright lines of greater and greater wave-length passed over into dark lines. This relationship between selective absorption and wave-length does not apply to all of the iron lines, for some of them remain bright even under circumstances highly favorable to reversal. But if we except such lines, and also certain others, including those of great intensity (which reverse long before the fainter lines in the same region), we may say that selective absorption begins in the ultra-violet and gradually advances into the less refrangible part of the spectrum.

In order to ascertain whether similar results can be obtained with other metals, with the assistance of Dr. Kent, I have photographed the spark spectrum in water of the following metals: titanium, magnesium, cadmium, copper, nickel, cobalt, lead, and aluminium. Speaking generally, and omitting reference to certain doubly reversed lines and other interesting details requiring special investigation, it may be said that the absorption phenomena of most of these metals are very similar to those previously found for iron: the first reversals occur in the ultra-violet, and only as the conditions become more favorable to absorption do dark lines appear in the less refrangible region. Some of the spectra contain very few dark lines, but the absorption phenomena of cobalt and titanium are fully comparable with those of iron.

It thus appears that in the case of some of the metals the law of selective absorption resembles that of general absorption. It seems probable that other elements will give similar results, particularly in view of Campbell's discovery that in the spectra of certain stars the ultra-violet hydrogen lines are dark, while those of greater wave-length are bright.[1] The effects of partial reversal which Campbell describes are very similar to those frequently obtained with the spark in liquids.

It should be added that in 1893 Professor Frost suggested an explanation of such stellar spectra, based on the assumption that the law of selective absorption resembles that of general absorption. In a recent number of this JOURNAL (December 1901), Professor Kayser has given a somewhat more detailed explanation, derived from a simple application of Kirchhoff's law.

YERKES OBSERVATORY,
    April 19, 1902.

[1] ASTROPHYSICAL JOURNAL, **2**, 177, 1895.

Minor Contributions and Notes.

EARLY OBSERVATIONS OF ALGOL STARS.[1]

One of the most important uses of the collection of photographs at Cambridge is to determine early dates of minima of stars of the *Algol* type. Nearly all of the early photographs were taken with the 8-inch Draper Telescope. The number has been greatly increased during the last three years by supplementing its work with anastigmat lenses. In the *Astron. Nach.*, **156**, 313, Mr. Williams announces that the star 78.1901, R. A. $= 20^h$ 18^m $4^s.0$, Dec. $= +42°$ $46'.4$ (1855), is a star of the *Algol* type. One hundred and seventy-seven photographs of this region were contained in the Harvard collection, the first being on September 19, 1885. On ten of these the star was distinctly fainter than its normal brightness. Measures with the twelve-inch Meridan Photometer gave the maximum magnitude of this star 10.47. Assuming this value, a light curve, whose coördinates are given in Table I, was determined by Professor Wendell from observations with the fifteen-inch telescope.

TABLE I.

LIGHT CURVE.

Ph.	Decrease.	Increase.
d		
0.20	10.47	10.47
0.18	10.55	10.50
0.16	10.67	10.57
0.14	10.81	10.75
0.12	11.07	11.03
0.10	11.15	11.11
0.08	11.45	11.38
0.06	11.80	11.75
0.04	12.22	12.16
0.02	12.76	12.64
0.00	13.05	13.05

The various determinations of the minima are enumerated in Table II. The year, month, and day are given in the first column, the

[1] *Harvard College Observatory, Circular* No. 64.

Greenwich Mean Time in the second, the Julian Day and fraction in the third, and the photographic magnitude in the fourth. The fifth column gives the time from minimum, indicated by the magnitude according to the light curve, assuming that the normal photographic magnitude is 10.2. On three of the plates the star was not seen, although stars of the magnitude 11.7 were visible. This indicates that the star was within 0^d06 of minimum. The last four lines relate to visual observations, three by Mr. Williams and the last one by Professor Wendell. The letter m is entered in the fourth column, and the value in the fifth is of course zero. The adopted formula for the times of minima is 2,410,000.20 + 3.45083 E, which agrees closely with that given by Mr. Williams. The value of E is given in the sixth column, and the deviation O–C in the seventh. These deviations must be corrected by the values in the fifth column, although they are sometimes thus increased. In the first line the correction may have any value from $+ 0^d06$ to -0^d06, but even the latter leaves the deviation $+0^d009$. In the second line the magnitude 10.83 shows that the time of minimum must have preceded or followed the time of observation by 0^d121.

TABLE II.

OBSERVED MINIMA OF 78.1901.

Y.	M. D.	G. M. T.		J. D.	Magn.	Curve.	E.	Unc.	Corr.
		h.	m.	d.		d.		d.	d.
1890	5 16	19	56	1504.831	< 11.7	±.006	436	+.069	+.009
1891	1 13	10	50	1746.451	10.83	−.121	506	+.131	+.010
1892	9 18	14	07	2360.588	< 11.7	±.06	684	+.020	.000
1893	7 29	18	04	2674.753	10.67	−.143	775	+.160	+.017
1894	9 23	14	03	3095.585	< 11.3	±.08	897	−.010	.000
1895	8 10	14	52	3416.619	10.98	−.107	990	+.097	−.010
1896	8 20	16	32	3792.689	< 11.7	±.06	1099	+.027	.000
1897	9 25	14	46	4193.615	10.86	−.120	1215	+.657	+.537
1898	1 13	10	43	4303.447	11.62	−.062	1247	+.062	.000
1899 10	3	13	43	4931.572	10.52	−.162	1429	+.136	−.026
1901	8 24	14	27	5621.602	m	.00	1629	.000	.000
1901	9 7	9	43	5635.405	m	.00	1633	.000	.000
1901	9 14	7	29	5642.312	m	.00	1635	+.005	+.005
1901 11	1	14	47	5690.617	m	.00	1649	−.002	−.002

It is by no means certain that the best value of the period, $3^d45083 = 3^d 10^h 49^m 12^s$, has been found. A change of one or two seconds would, however, increase the residuals perceptibly. It is curious, and perhaps suspicious, that the correction has always been taken with the negative sign, that is, that the star when photographed was in each of

the ten cases increasing in light. One result, that on J. D. 4193, seems entirely wrong. The minimum apparently took place half a day too late. Some of the large residuals occur when the star is bright, or when it is changing slowly. We should expect that they would be the most uncertain. It is probable that more accurate results can be obtained when the photographic light curve is better known, and also by correcting for aberration.

In the *Astron. Nach.*, **157**, 79, Dr. Schwab announced that the star $+19°3975$, 93.1901, R. A. $= 19^h 14^m 26^s$, Dec. $= +19°25!4$ (1900), was a variable of the *Algol* type. From the *Harvard Annals*, Vols. XXIV and XLV, it appears that the photometric magnitude of this star when at full brightness is 6.50, the range in the measures on four nights being less than a tenth of a magnitude. An examination of the Harvard photographs showed that we had 155 images in which its brightness was nearly normal, and thirteen in which it was near minimum. For the observations near minimum, Table III gives in successive columns the year, month, and day, the Greenwich Mean Time, the Julian Day and fraction, the approximate magnitude, the assumed valued of E, and the uncorrected residual found by subtracting the time computed by the formula, 2,410,011.6 + 17 E.

TABLE III.

OBSERVED MINIMA OF 93.1901.

Y.	M.	D.	G. M. T.		J. D.	Magn.	E.	Unc.
			h.	m.	d.			d.
1887	8	3	15	35	0487.649	7.06	28	+0.05
1890	11	5	10	48	1677.450	9.15	98	−0.15
1891	7	13	16	28	1927.686	8.68	113	−4.91
1893	8	4	15	2	2680.626	7.06	157	+0.03
1893	8	4	15	14	2680.635	7.06	157	+0.04
1894	10	15	12	8	3117.506	7.35	183	−5.09
1895	8	22	13	22	3428.557	8.74	201	−0.04
1895	8	22	14	25	3428.601	8.11	201	0.00
1895	9	8	14	22	3445.599	8.40	202	0.00
1897	9	24	13	31	4192.563	7.06	246	−1.04
1900	5	22	19	33	5162.815	7.80	303	+0.22
1901	5	12	19	4	5517.794	8.74	324	−1.81
1901	7	12	17	13	5578.713	9.15	327	+8.11
1901	7	12	17	28	5578.728	9.11	327	+8.13
1901	10	8	13	14	5666.551	8.68	333	−6.05
1901	11	1	6	30	5690.27	m	334	+0.67
1901	11	28	12		5717.50	m ?	336	−6.10
1902	1	4	12		5754.5	m ?	338	−3.10

The photograph on J. D. 3428 is especially valuable. The spectrum, which is of the first type like other *Algol* stars, trailed over the plate, and showed that the star was at first of about the ninth magnitude, after about half an hour suddenly becoming brighter until it attained the eighth magnitude at the end of the exposure. Two lines have, therefore, been given to this plate. When the law of variation becomes known this will probably give an accurate value of the time of minimum. The last line but two represents the observation of Dr. Schwab, the last line but one that of Professor Wendell. On this last date, the star had apparently nearly recovered its full brightness, the range of the observations not exceeding two-tenths of a magnitude. The last line represents a somewhat uncertain observation by Mr. White when the star was near the horizon. The range in light appears to be greater than that of any other *Algol* star. The period, 17 days, given by Dr. Schwab, is longer than that of any other *Algol* variable, and does not satisfy the observations. No subdivision, or other value of the period, appears to give better results. By a slight change in the period, and by applying a correction for the light curve and for aberration, about half of the residuals in the last column of Table III could be reduced nearly to zero. The others are so large, amounting to several days, that their careful study becomes a matter of very great importance. If these discordances are due to a third body this star will become one of the most interesting in the sky. Evidently it should be carefully watched. This has been done at Cambridge ever since its variability was announced. On two dates, November 18 and December 22, 1901, when minima were expected, according to the formula of Dr. Schwab, clouds prevented observations, and on a third day, December 5, 1901, no diminution in light was perceptible. During the coming year it is proposed to look at it early and late on each clear evening at Cambridge and Arequipa. The value of this work would be greatly increased if observers in other longitudes would co-operate. A continuous watch might thus be kept upon it, and no minima missed. The observations needed are very simple. The star when at full brightness is easily seen with an opera glass, and it is only necessary to select two adjacent stars, one a little brighter, the other a little fainter, as $+ 19° 3956$ and $+ 18° 4043$, and see each night if the brightness is normal. Any observations when the star is faint will be very valuable. If the observer is not accustomed to the method of Argelander, it will only be necessary to name one or two stars which

are a little brighter and others a little fainter at a given hour and minute. These observations should be repeated two or three times an hour to detect changes. The stars may be identified by a sketch of the region, if desired. The value will be greatly increased if any kind of photograph can be taken, attaching a hand camera to an equatorial or even allowing the stars to trail through the field. The detailed observations should be forwarded at once by mail, and if possible the time of minimum cabled. For instance, "Observatory, Boston, Schwab twenty-first sixteen, Smith," would be understood to mean that Mr. Smith found Schwab's variable at minimum on the twenty-first of the current, or preceding, month at sixteen hours after Greenwich Mean Noon. The message sent from here, "Schwab, May twenty-first," would indicate that a minimum was predicted for that date, and that observations on that evening were greatly desired. It is hoped that by the aid of astronomers in Asia and Australia it will be possible to follow the star continuously during the present year, so that no minima will be missed, or if so, only during a limited period, as the month of July, 1902, when the star is in opposition to the Sun.

EDWARD C. PICKERING.

JANUARY 18, 1902.

NOTE.—The following observations of Schwab's variable have been obtained at the Yerkes Observatory by Mr. J. A. Parkhurst:

Y.	M.	D.	Gr. T.		J. D.	Mag.		
			h.	m.				
1902	2	3	23	40	5784.986	8.31	vis. comp's	5 stars
1902	2	3	23	50	5784.993	8.69	photometric	2 stars
1902	2	4	0	28	5785.019	8.34	photometric	2 stars

The photometric results depend upon the HCO Meridian Photometer (Vol. XXIV) magnitudes of

$$
\begin{array}{ll}
& \text{M.} \\
\text{B. D.} + 19°3971 & 9.23 \\
19\ 3972 & 8.20
\end{array}
$$

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